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T. O'B. HUBBARD AND J. H. LEDEBOER, B.A.

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All communications should be addressed to—

THE EDITORS, 53, Victoria Street, Westminster, London, S.W.

Telegrams: "DIDASKALOS, LONDON" Telephone: 3656 VICTORIA

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Notices of the Aëronautical Society of Great Britain

GENERAL NOTICES

February Elections

R. R. à Court Beadon — A. A. Crawford — Oscar C. Morison
R. Bottomley — T. Kimpton — C. J. Partridge
A. Mortimer Singer

March Elections

B. G. Cooper — Mervyn O'Gorman

April Elections

R. F. Macfie — E. C. R. Marks

The first two numbers of the Aeronautical Classics, reprinted by the Society, have been published. They are "Aerial Navigation," by Sir George Cayley, Bart., and "Aerial Locomotion," by F. H. Wenham. Two further numbers, dealing with T. Walker, Stringfellow, and Pilcher, are in preparation, and will be published in June.

The following telegram was despatched to the Berlin Aero Club on the occasion of the death of Lt.-Col. H. W. L. Moedebeck:—

> "The Aëronautical Society of Great Britain deplores the irreparable loss to aeronautics of Lieutenant-Colonel Moedebeck, and pays sorrowful homage to the memory of a profound scientist and a gallant officer."

COMMITTEE NOTICES

The *Experimental Ground Committee* will be glad to receive the opinions and suggestions of all Members with regard to a flying-ground. Communications should be addressed to the Secretary of the Committee at the Society's Offices.

The *Laboratory Committee* has been strengthened by the addition of Mr. Mervyn O'Gorman to their number.

The *Consideration of Inventions Committee* has drawn up the following notice to inventors, which has been approved by the Council:—

NOTICE TO INVENTORS

The Council of the Aëronautical Society of Great Britain are prepared to examine proposals from inventors with regard to aeronautics. In sending in proposals of the kind, inventors should clearly understand that the Society undertakes no responsibility whatever in connection with them, and it must also be understood that they are only considered on the express condition that no report or communication emanating from the Aëronautical Society in the reply thereto should be used for the purpose of advertisement or publication, without the written consent of the Council. Any proposals should be addressed in the first instance to the Council, who will submit them to Committees nominated by them, for the purpose.

Inventions will be considered in the order in which they are received, and all communications will be regarded as strictly confidential.

No fee or charge of any kind will be made. Copies of documents only should be sent, and not originals.

With regard to models, the Council will not hold themselves responsible for any damage sustained by them.

LIBRARY NOTICES

The Council desire to thank the following donors for their gifts, including furniture, books, prints, and lantern slides: Mr. P. Y. Alexander, Mr. W. Bennett, M. D'Estournelles de Constant, Herr. J. Hofmann, Herr B. Wilhelm, M. Emile Wenz, *l'Aero*, and the Royal Society of Arts.

1. The following books have been added to Library in addition to those mentioned under "New Books and Publications":—

D'Esterno, M.	*Du Vol des Oiseaux.*	1865.
Marey, E. J.	*Le Vol des Oiseaux.*	1890.
Mouillard, L. P.	*L'Empire de l'Air.*	1881.

ANNUAL GENERAL MEETING

The Annual General Meeting of the Aëronautical Society of Great Britain was held at the Royal United Services Institution, Whitehall, on Friday, March 18, 1910, at 7.30 p.m. The President, E. P. Frost, J.P., D.L., occupied the chair, and was supported by Colonel J. D. Fullerton, Colonel Tyler, Major Baden-Powell, and Messrs. J. Ward, W. F. Reid, and E. S. Bruce.

A considerable number of Members were present.

The proceedings opened with the reading of the Report of the Council of the Society by Mr. E. S. Bruce:—

COUNCIL'S REPORT, 1909

The Council congratulates the Society upon a prosperous year, during which its membership, revenue, and activities have largely increased.

At the beginning of 1909 new and larger offices were acquired and permanent staff constituted. These offices are already beginning to prove inadequate, and it is hoped that further accommodation may be available shortly.

An agreement was concluded in May with the Royal Aero Club and the Aerial League, whereby the spheres of the different bodies were defined and recognised, this Society being regarded as the paramount scientific authority on aeronautical matters.

An experimental Flying Ground, the first to be acquired by any body, was obtained at Dagenham, where some interesting work has been carried on, but as it has proved inadequate for the needs of the Society, it has been abandoned—though this has meant some financial loss to the Society. The Council felt that it would be better to obtain a good ground elsewhere than to continue to spend money on one that was unsuitable.

A remarkably fine exhibition was made by the Society at the Travel Exhibition in July, but owing to the little interest taken in this Exhibition, it did not meet with the success it deserved.

The Society has awarded its Gold Medal to the Messrs. Wright, and its Silver Medal to Mr. Cody, for their valuable work in the interests of aeronautical science. The Bronze Medal given annually for the best contribution in the *Aëronautical Journal* was awarded for 1909 to Mr. F. W. Lanchester for his paper on the Wright and Voisin types of flying machines.

Many Committees have been recently formed in the Society, all of which should do valuable work.

The appeal which appeared recently in the Press on behalf of the Laboratory Committee has met with some response, and it is hoped that this Committee, perhaps the most important of those at present constituted, will shortly be in a position to begin the practical and experimental side of its work.

The Council takes this opportunity to appeal to each individual Member of the Society to assist in increasing the Membership. With the daily increasing work and the spread of the Society's influence, it is necessary to have large financial resources in order that the efficiency and dignity of the Society, the oldest aeronautical body in the world, may be adequately maintained.

Colonel J. D. FULLERTON: The financial statement has been sent out to all the Members, and as you have, no doubt, all seen it by this time, I will, if I may, make a few remarks on the subject explaining perhaps a few items here and there. This year, of course, has been a very difficult one for the Council. You may say it is the first year that this Society has had anything in the way of

funds to do any work, and a great many things have been started and a great many new things taken in hand. Thanks to the kindness of Mr. Patrick Alexander, we were enabled to take much better offices (applause), and that, I think, has been of very great assistance to the Society. There was a very good Show at the Travel Exhibition, but unfortunately the public generally did not take to the Exhibition at all, and what I think was one of the best Shows I have ever seen at any Exhibition was practically wasted. I think it brought us a few members, people who really were interested in the subject, and I think it was of some assistance to us in that way. As regards printing and stationery, that sum, I may say, includes the cost of the monthly circulars. As you may recollect, when I first became Hon. Secretary, we started the system of sending out circulars about once a month giving information about various things that were going on, and so on, to the members. I think the members seemed to like that, but, of course, in a small society like this we cannot get them printed, we have to have them all typewritten. It is a certain amount of expense but on the whole, as far as I can ascertain by speaking to members on the subject, they seem to like the system, and I think it should be continued. Postage, of course, has gone up very considerably. It is more than double what it was the first year I was Hon. Secretary. The item for office expenses and so on, £93 6s. 11d., includes the pay of typewriters employed in the office. The *Journal* has done a good deal better this year, I am glad to say. Last year, I think it was, we only made about a third of our expenses in publishing it. This year we have made about a half, and I believe that, as scientific journals go, is rather satisfactory; most of them, I believe, are a very considerable loss. As regards the experimental ground—the ground, unfortunately, was not taken up as well as we had hoped. I hoped there would have been more members working down there, and that we should have been enabled to clear a greater space and make the whole place into a really good ground. The difficulty, of course, is that, with only a very moderate income, you cannot afford to clear a place like that, but I think if we had had a lot of members down there, and could have spent some money on clearing, we might have made a better thing of it. However, that, unfortunately, cannot be helped. There is one point I should like to allude to that we did, and I think it is a very important one. We made—I believe it is the only time it has ever been done anywhere in any of these experimental grounds —a good starting-track, it was about a quarter of a mile long and forty feet wide. Our intention was, if we had had more money, to extend that in different directions so as to be able to start against the wind in any direction. Unfortunately we were not able to do it. As regards membership—the membership, of course, has gone up very considerably; it has more than doubled (applause), and that is very satisfactory. One item I might refer to in the Balance Sheet: the office furniture, aeroplanes, printed books, etc., as per last Balance Sheet, £40. That was only a temporary valuation that I made. I believe that those particular things are worth a very great deal more, but it is a question whether it is worth while spending the money and getting a regular valuer in and absolutely valuing all these things or not; on the whole I thought at present it was not, but later on we might do something of that sort and get a regular valuation of the whole of the Society's property made, and I think in the present state of affairs it is hardly necessary. The remaining items do not call for very much comment.

The PRESIDENT: I do not know whether any gentleman has anything to say. I should like to know if you adopt the Report which you have heard read.

Mr. HOLTORP then proceeded to criticise the Balance Sheet at some length.

Mr. WARD: Gentlemen, as Honorary Auditor to the Society, perhaps it is only proper I should endeavour to clear up the questions that have been put. I have made very rough notes of them. On the question of the Balance Sheet generally I might state that as a chartered accountant, when I was appointed to be Honorary Auditor of the Society, I was not at all satisfied with the way the accounts

had been made in the past. I suppose that is only natural for a professional accountant coming fresh into contact with accounts that previously had been made up, if I might respectfully suggest it, by amateurs, and therefore I did my best to place these accounts upon such a basis that they would work satisfactorily for the future. That being so, of course, it is very difficult for the lay mind to trace the connection between these accounts and the former year's. I had to a very great extent to dissect anew last year's accounts in order to arrive at a satisfactory basis on which to found this year's accounts. Therefore I quite agree that it will be very likely difficult on the last year's accounts for you to trace some of these figures which are stated as per last Balance Sheet. But in the aggregate they agree with the balance of £225 to the credit of last year's Balance Sheet. There was nothing fundamentally wrong with last year's Balance Sheet except it was not dissected under the various funds to which it should have been appropriated. Now, in this year the Society owns certain assets of which, of course, the cash at the bank is obvious, and the cash in hand. The office furniture, aeroplanes, etc., were valued last year, I believe, on the Balance Sheet at £40. That figure is brought forward into this year on the asset side, and to that is added the capital expenditure for the year, the actual additions apart from the re-binding and repairs, is £36 17s. 0d.—the actual additions to the Library and Assets to the Society for this year. Against that is written a small amount of depreciation giving a round figure of £70 as the current valuation; that is, assuming the valuation last year, £40, to be a fair one. The actual expenditure this year has been added to the small depreciation deducted, so the figure of £70 is a fair figure to put down in this year's Balance Sheet from a revenue point of view. The item appears in the General Income and Expenditure Account under the heading of Library Expenditure, that, of course, is on re-binding, and what we should call in ordinary commerce, repairs; it has no connection with the £36 17s. 0d. expended on actual additions to the Library. I don't know whether I have covered your points.

Mr. HOLTORP: The estimated value of buildings on experimental ground.

Mr. WARD: Well, whether a sale has actually taken place or not, I was informed, in putting that figure in these accounts, that if the buildings were sold it was what they would realise. Perhaps the Council may be able to give you further information whether they have been realised, but as I understood the ground was going to be removed I made inquiries as to what the value of those assets would be for removable properties. If there had been no contemplation of giving up the ground, I should have been justified in putting those assets in at cost price, subject to a reasonable depreciation for one year, but as I had information that the grounds were going to be given up, I asked whether those various buildings were worth removing, and I was informed they were not, but if sold they would realise £40; that is my justification for putting them as an asset on the Balance Sheet.

Colonel J. D. FULLERTON: One of the things I have always thought was that the Society should have its accounts audited by a professional accountant. Mr. Ward very kindly offered to do this, and I think it is a very great advantage to us. Of course, up to last year the accounts were all on a small scale, and they were done certainly, as he says, in an amateur way. We did our best, but we don't profess exactly to be up to the standard of a professional accountant, and I thought—it is my own special recommendation—that we should get a chartered accountant to do them all and to put them in a form which would satisfy the whole public generally, and the Society at large. (Applause.)

The PRESIDENT: I hope, Gentlemen, that the explanation will be considered quite satisfactory and that we may adopt the Report.

On the motion of Colonel J. D. Fullerton, seconded by Colonel Tyler, it was resolved: That the Report of the Council and the Balance Sheet for 1909 be adopted.

The PRESIDENT: The next on the Agenda is a motion to be proposed by Mr. C. C. Turner.

Mr. C. C. TURNER: Mr. Chairman, Ladies and Gentlemen, the resolution of which I have given notice is a follows:

> "In consideration of the satisfactory progress of the Aëronautical Society and of its largely increased activity, and in view of the situation revealed in the Balance Sheet with reference especially to the Experimental Ground Account, it is resolved that an Executive Committee be constituted, its members to be elected by the members of the Society, and half of their number to retire annually, the members of the Committee being elected for two years, the purposes of this Committee being to relieve the Council of a portion of the routine work and details of management which should now be borne by the members of the Society themselves."

Now, my object in putting forward this resolution is that it seems to me that, owing to the greatly increased activity of the Society and the great increase of business thrust upon it, it is very necessary that the organisation should be more efficient than it has been during the last thirty or forty years. This is not my discovery. Colonel Fullerton, when he resigned from the Honorary Secretaryship last year, read a paper before the Society on the work of the Society, in which he proposed certain changes which seemed to me to be very desirable. Among those changes was the formation of a General Purposes Committee, whose work would be to take from the shoulders of the present Council a great deal of the burden of the business affairs. Now, I should say, perhaps, before I go further, quite clearly that this resolution must not be taken in any way as an aspersion upon the present Council. (Hear, hear.) It is a resolution justified, if justified at all, by the accidental development of the work of the Society. Now one or two things lately have brought this very prominently to my notice. For instance, in the first place there is the experimental ground. To my mind, although the accounts may be in a perfectly satisfactory state, there still remains something to be said. The experimental ground at Dagenham evidently was not suited to its purpose. I should very much like to know who chose that experimental ground, whether it was chosen by all the members of the Council in a body, or whether any properly constituted Committee visited the ground and decided upon taking it. From what I can gather, the ground was used by one or two members of the Society only, yet the expense of the ground, apart from the donations for that special purpose, has, I take it, devolved upon the funds of the Society to which we all contribute. I understand that a new experimental ground is to be chosen, and I think clearly that in this case the Society is entitled to have some satisfactory assurance that the choice of this ground will be made in a very careful way indeed, so that there will be no repetition of the fiasco—I think one might almost describe it as that—attaching to the Dagenham ground. There is one other point which occurs to me. I was at a meeting of the Aerial League in the City the other day, and something happened there which seemed to indicate that in the opinion of other aeronautical bodies and societies, this Society is not fulfilling its functions. An appeal was made for funds for the establishment of an experimental laboratory for scientific research by the Aerial League. The President of the Aëronautical Society was on the platform at this meeting, and not only did he not protest against this usurpation of the functions of the Society, but he actually supported the appeal. I cordially agree that a proposal for an experimental laboratory likely to be largely supported by public funds deserves every possible assistance, but at the same time it was agreed early last year by the Aero Club and the Aerial League and the Aëronautical Society that these three bodies should control different divisions of aeronautics, and the sphere of influence allotted to the Aëronautical Society was the scientific one. I certainly think that that livision is far safer in the hands of this Society than it is in the hands of the Aerial League or the Aero Club. (Hear, hear.) The time at our disposal is very short as we are to have a lecture to-night by Mr. Headley, so

I will not make any further remarks, but I am quite certain that there are a number of arguments which, if they were ranged alongside, would convince members of the Society of the need of an elected committee to take over this part of the work, to attend to the numerous details which crop up almost daily, and to relieve the Council, who are for the most part eminent scientific men; who ought not to be troubled with small petty business details and little things of management; whose whole attention should be concentrated upon the high policy of the Society. I think this meeting ought not to disperse without coming to some definite determination upon that point. (Applause.)

Mr. C. A. ALDERSON: I have great pleasure in seconding this resolution which Mr. Turner has brought before the meeting. It is not enough for us to ponder calmly over scientific problems, as has been the course of the Society in the past. Nowadays we see springing up around us numerous similar associations, and therefore it is necessary for us to amend our position, to adopt a more commercially-minded and pushful method of procedure, so that we may hold our own. Surely, with the records of experimental work and history, scores of years longer than that of other societies—provided we adopt such a method of working as has been suggested in Mr. Turner's resolution—we ought to hold our own. The time has at last arrived when engineers, and, indeed, everyone else, are taking aviation seriously. Engineers would preferably join our Society rather than those societies whose professed object is sport or mere model making, hence I think it is our duty to cater for such, to make our Society attractive to them, and the best way to do this is to adopt some resolution, such as has been proposed, to form a committee who would work upon business lines. The time is rather too short to go into full details of everything that ought to be considered at this meeting as we have a meeting coming on at 8.30, and many other speakers. Therefore for the present I shall not continue, but beg to second Mr. Turner's proposition. (Applause.)

Captain F. W. MARRIOTT: As a member of the Society I am quite with the speakers in the aims and objects they have related, but as a member of the Aerial League I object to the strictures which have been passed upon that League. It was perfectly within the rights of the League at the Mansion House at the Lord Mayor's meeting. I think it is a very despicable state of affairs that brings great societies like the Aëronautical Society of Great Britain, the Aerial League of the British Empire, and the Aëro Club into conflict in the manner which has been the case for the last year, or perhaps a little less. It would be much better if we allowed each Society and each League to go its own way, and do what it thinks best, and if it is successful in the methods it takes to let it reap the reward of that success. I think this petty way of finding fault and saying, you are getting in my way, this is not your province, and that is not your province, is —I cannot use any other word—contemptible. I don't see in the least why, just as great companies run along their own lines, get their own land, start their own financial course, we cannot do it in the same way. I never want to know what the other societies are doing. I do what I consider will develop the science which I am taking an interest in, and doing what I can to promote it. Our province is the air, and the air is open to us all. Let us each in our own way develop the science, and make the progress we can, and our own success will be our justification whatever direction it may take in the matter. I have nothing more to say.

Mr. H. TURRILL: I should like, also as a member of the Aerial League, to say that I think Captain Marriott misunderstands the position The Aerial League, the Aero Club, and the Aëronautical Society are mutually helping each other.

Captain MARRIOTT: Why such a stricture as we received just now?

Mr. C. C. TURNER: May I make one word of explanation. I did not intend to say anything which reflected in the slightest degree upon the Aerial League, but early last year, I think I said in my remarks, an agreement was come to

between the three principal aeronautical societies which relegated to the Aerial League the patriotic and educational work, to the Aëronautical Society the scientific work, and to the Aero Club the domain of sport, and it is entirely in accord with that agreement that my remarks were made or intended. (Applause.)

Mr. R. M. BALSTON: Mr. President, may I say a word, as a member of the Ground Committee at Dagenham, on the strictures that were passed on that Committee by Mr. Turner, calling it a fiasco and all that sort of thing. May I say I think he treads on rather dangerous ground in saying anything of the sort, because everything that possibly could be done to get a good ground was done, and it was recognised at the time that it was not easy to find a ground because you did not know exactly what was wanted. A great deal of trouble was taken that no contract should be entered into which should tie the Society down to any particular spot because of this difficulty. The ground has proved, like practically every other ground in the kingdom, to be a failure, therefore it has been given up. I think Mr. Turner will find that this has happened in every other case so far.

Mr. S. F. CODY: May I say a word also as a member of the Society about this ground question? I should always be quite willing to assist the Society in choosing a ground. I think I may say that I have some little experience in what a ground should be for aviation, either for a skid machine launched from rails or for a practical wheel machine, and I would assist the Society in any way I could. But unfortunately no Society or Club in Great Britain has asked me to give my opinion on any such thing. If they did so I would give it willingly. I am a member of the Aero Club and the Aeroplane Club as well, but I have never been asked by any of these Societies what would be a suitable ground for practising flying. If such an honour was paid me I should be glad to render my service in that way. (Applause.)

Colonel J. D. FULLERTON: I have been desired by the Council to move the following amendment:

> "That in consideration of the satisfactory progress of the Aëronautical Society, and its largely increased activity, it is resolved that the Council be requested to consider further the question of appointing Committees to relieve them of a portion of the routine work and details of management of the Society."

I do not wish to say very much, but as Mr. Turner quoted my proposals I may perhaps make a few remarks. My object was simply to help the Council in getting rid of a great number of minor details which I think could be very well carried out by a smaller Council which I call the General Purposes Committee. I think that perhaps is the most convenient name. I do not want that Council or Committee, or whatever you like to call it, to interfere with the other Committees. All the other Committees, naturally, like to have a say in their own affairs. and my idea was that it should not in any way interfere with their work, but just let them do their own things in their own way. As regards the experimental ground, there was a regular committee which chose the ground and went into the whole thing most carefully and took a great deal of trouble about it. They went all over the place and tried all sorts of grounds, and I do not think they can fairly be blamed for things not working out very well. As regards the Aerial League, I am very sorry that there should be any discussion or unpleasantness about the subject. We are very anxious to work satisfactorily with everybody, and we have made our agreement, and I think we are all ready to stick to it. As regards the laboratory, I understand what they want is more for purely educational purposes and not the sort of laboratory that we want, which would be, and I hope will be, a big affair and really a very important institution. I attach the very greatest importance to a laboratory, for it is the thing that is wanted more than anything else, in my opinion (hear, hear), and I do hope that we shall some day get funds to do the work on a proper scale. It seems to me

this aeronautical engineering question should be taken up by men who really understand aeronautics. (Hear, hear.) It is not only an engineering question; there is a great deal of aeronautics mixed up in it, and I think this Society the proper Society to do that kind of work. Of course, the question is one of money, that is what has always hampered this Society all through its career. If you read up the accounts of the Society and of its proceedings, and so on, you will see that there was plenty of good work done, heaps of things that were really most praiseworthy in every way, but the Society has always been hampered by the fact that it could not get the money to do the work; and in some way or other, if we are to keep things going properly, we must get money to do it. The experiments are very expensive, and they require not so much the actual instruments and appliances, but a great deal of time, and you must employ people who can be regularly at work. Of course, members help a great deal, as they always have done, but still you must have these men employed constantly and regularly and properly paid. I am particularly keen about this laboratory, because I believe it is the very thing the Society ought to be doing, and I hope we shall some day get funds to do it. (Applause.) As regards the engineers joining the Society, I am most anxious they should do so. I may say that the Council are considering this very subject of how best we can attract them.

Mr. Eric S. Bruce: I beg to second the amendment proposed by Colonel Fullerton.

Mr. Massac Buist: It seems to me that in the proceedings so far to-night, there have been revealed to us some of the advantages and some of the disadvantages of a tradition. We inherit very great traditions. The system on which the Society has been run in the past is the only possible system on which it could have been run then. But we are now coming up against the fact that the work of the Society is developing in many directions. For instance, we find points are being raised concerning what has been done by the Council of the Society. I take it that none of us have any feeling about those points. We are regarding them merely as illustrations. That is my own point of view anyway. As regards anything that may be said about the grounds, I was interested in them, and, like most members, I subscribed to them. I am very sorry they should not have been the great success anticipated. It seems to me that we who compose this Society have allowed the Council to take the whole burden on its shoulders. It has done all the giving, we all the taking. It is a very awkward position for members ever to have occasion to make any remarks on what has been done for them in such an ungrudging fashion, while they themselves have done absolutely nothing towards sharing the responsibility which the Council has in any given matter. Surely whenever the membership might have occasion to differ with what the Council should decide to do on any given occasion, the members themselves should be in such a position—or should put themselves in such a position—that they take the blame themselves, if there is any blame at all. If the members agree to elect—were themselves yearly electing—some sort of executive committee to look after those questions about which there might be differences of opinion, or where financial responsibility would be involved or anything of that sort, I certainly think any mismanagement would recoil on our own heads, and not on the Council. We should not put the Council, or allow it to remain, in such an embarrassing position. Our numbers are to-day very small, but our traditions are very great. We also see around us very powerful aeronautical bodies rising quickly. Presently they will swamp us, unless we put ourselves in a way to become really as big as our name would indicate and the public imagine us to be. We are talking in privacy among ourselves, otherwise I should not touch on the matter in that light. I think before we part to-night it is a serious question for the members to consider as to whether in some form or another—for example, using Mr. Turner's resolution as a convenient instrument—they could not give the Council some expression of their opinion to let them understand that, instead of being utterly indifferent and having everything done for

them, they are willing in a sense to share the responsibility of the Council in taking on themselves the routine work which incidentally involves responsibility of a financial nature.

Lieut.-Col. H. R. C. Cross: I must apologise first of all for coming late. I had not the advantage of hearing the speeches made at the beginning, but I may say that my object in joining the Society was that I felt that, as a member actively engaged in aviation work, I was not doing my duty towards the Societies or towards the Country in not giving time and money to the purposes for which we are gathered to-night. I would like to suggest that a way out of the difficulty might be to appoint a committee amongst the members to-night to talk over matters with the Council to see how the Council can be helped and what is of the most use. I quite agree, for instance, with regard to the question of the laboratory, and I for one would be very glad to give ten guineas towards it (applause), and I think we might start a fund to get the money for it. I think there are many of us who have friends who would subscribe if we wrote personally to them and pointed out the objects to them. There is no doubt that, as the last speaker said, unless the ship is well handled we have got a chance of being swamped by the other societies. I, for one, would certainly back up the Council in anything that they recommended, and I think a good plan would be to appoint a committee to discuss the whole matter. There is no doubt any working in opposition between the members and the Council is absolutely hopeless—we must work together. (Hear, hear.)

Mr. J. H. Ledeboer: It seems to me that Colonel Fullerton, at any rate, is labouring under some slight delusion in his grasp of the scope of this committee which Mr. Turner proposes should be elected by the members of this Society. Obviously, it would not and could not interfere with the work of any of the other committees. Its work as a committee would be entirely separated from them. It would deal solely with the business of the Society and nothing else. The work of the other committees would be kept to them, and their report would not be made to the Executive Committee but to the Council and to the Society, and not to any other committee.

Colonel Fullerton: Do you mean that they would not interfere in any way whatever, say with some other committee for the consideration of birds or anything of that sort?

Mr. J. H. Ledeboer: Not at all; the idea of an Executive Committee is simply a committee that can deal with the business affairs of the Society. The scientific work of the Society is altogether outside its scope.

Colonel Fullerton: That is the point I want to make. They should not interfere with any of the other committees. I do not think the other committees would like it.

Mr. J. H. Ledeboer: It is not its business to interfere with them. In the second place, I think it would only be creating delay if we were to appoint a committee of the members to meet the Council. This is a very satisfactory meeting as far as numbers are concerned. The members are here now, and I think they are fully qualified to judge for themselves, and come to a decision at this very minute. The proposal itself seems to me to be a perfectly fair one. With the exception of solely scientific societies who have practically no funds to handle and have certainly no forward policy such as we have, every club and every society has an Executive Committee, and that Committee is elected by the members and is constituted on some such general lines as are indicated in this resolution. It would only be a great delay, and delay in this case may certainly be dangerous, if we put off the matter, and I think the Society, as represented by the members present, ought to come to a decision straight away, and to-night. (Applause.)

Mr. W. F. Reid: I should like to support the amendment of Colonel Fullerton. There seems to be no question whatever from the wording of Mr. Turner's resolu-

tion what he means. The committee is an executive committee. There is no question whatever as to the meaning of the word executive. It is a committee which has the power of action and the power of carrying out things in connection with the Aëronautical Society. It is of no use talking about words and the meaning of words—the thing is so perfectly plain that there can be no doubt as to it. I do not think Mr. Turner will deny for a moment what the meaning of the word executive is. Now the executive of nearly every scientific society is the Council and has to be the Council. I do not know of a single scientific society where the executive is not the Council.

Mr. J. H. LEDEBOER: May I just ask, in the majority of the other scientific societies is the Council elected or not by the members?

Mr. REID: That is so, and there is a resolution down. You will see on the paper that a resolution is to be put as to the mode of election of the Council of this Society. This Society was founded many years ago, and I do not know, I do not recollect at any rate, any society at the present moment where the members of the council are not elected by the members of the Society. They ought to be elected by the members of the Society in my opinion, and that is I believe a custom in most societies. I am a member of at least a dozen scientific societies, and in nearly every case there is an obligatory change of members of the council, after a certain number of years' service. Generally after 3 or 4 years a certain number of the council retire, sometimes by seniority, sometimes by the number of attendances, and I personally think that our Society ought to be treated in the same way, and I believe that is the object of the resolution which is to be put to you. Now what has been suggested as the object of Mr. Turner's resolution—the relieving of the Council of detailed work of the Society. That is being done to a large extent already. I believe Mr. Turner himself is a member of one of the committees, and you have had a notice, I believe all of you, asking you to volunteer to serve on these committees. These committees are doing excellent work. I understand that what is wanted by Mr. Turner is an active share in the management of the affairs of the Society outside the Council. Well, gentlemen, I really do not see how that can very well be done. A house divided against itself cannot stand, we all know. If you do not call it an executive committee what do you mean? Something else? But you call it an executive committee. It must be outside the Council and elected by the direct votes of the members of the Society. There must be clashing under the circumstances. The Council welcome every possible advance that the members may make towards making the Society more efficient—we must keep up to date—but I do think it is a little disappointing to the members of the Council. Some time ago we were the first to start an experimental ground, we went ahead, and what is the result? We get blamed for it. We happened to lose a little money over it, and we get blamed. It is not fair at the present day to say what ought to have been done two or three years ago in aeronautics. Now we know very well what an experimental ground ought to be. But at that time we were pioneers in experimental ground and started the very first; you cannot blame anybody for being a little wrong in their estimate of the requirements of such a ground. We did the very best we could and went ahead and got a place which had certainly some very good features—I won't go into details—but on the whole it was found to be unfavourable. I think it is hardly fair to criticise the Council in that way when they go ahead and do the thing. You cannot expect a profit from a scientific society. You must expend money, you have got to. We are not a commercial society either; we cannot expect to go in for the same things as perhaps the Aero Club would; we are a scientific society. We are making a great number of experiments in connection with aeronautics, and if we had more funds we should make more. I think that the best interests of the Society will be served by passing Colonel Fullerton's amendment to the original resolution, and I am quite sure that the Council will do everything they can to undertake that members who are willing may work on those committees which are being formed.

With regard to the constitution of the Society, which in my opinion is the cause of the whole difficulty, the Council are themselves taking steps to alter that in the direction which I have indicated, and I think with that you meet every requirement of the members and bring the constitution of the Society up to date. I have every pleasure in recommending the amendment to the meeting.

Mr. J. R. PORTER: I should like to ask one question, and that is, how is the Council of the Aëronautical Society elected?

Colonel FULLERTON: By the members of the Council. The existing members go into the whole subject, and discuss the whole matter, and if they are satisfied that he will be a good member of the Council they elect him; if not, they reject him.

Mr. J. R. PORTER: Is it not a very unusual proceeding?

Colonel FULLERTON: It is a proceeding that has been in force for the last 45 years.

Mr. J. R. PORTER: Out of date, I am afraid.

Colonel FULLERTON: I am not so sure of that.

Mr. E. A. DAVIES: I rather gather that if the Council were an elected body they would then be Mr. Turner's executive committee, and the whole dissolved into one. It is a very big question which we cannot settle right off in a few minutes just now. It has already been suggested that a few members might be elected to discuss the matter with the Council, and I should like to second that proposal. It seems to me the most practical thing that has been suggested to-night.

Lieut.-Col. H. R. C. CROSS: If the Council approve of that, I will put it in the form of an amendment.

Mr. J. H. LEDEBOER: Might I just make one personal explanation, Mr. President? Mr. Reid, in following me obviously laboured under the misapprehension that it would meet with the views I expressed on the resolution, if the Council were to be elected. To my mind that is a proceeding which is altogether wrong. We are a scientific society. There are very few men in this country who are qualified with a knowledge of the science of aeronautics to sit upon the Council of the Aëronautical Society, a position which is a very great honour. I think therefore that it would not meet either with the views or with the dignity of the Society if these scientists should submit themselves for election. (Hear, hear.) The Council of the Aëronautical Society is a very distinguished body, which has rendered very great services in the past, and ought to remain as it is constituted at present, but I think the members of the Society have a right to govern themselves. (Applause.)

Lieut.-Col. H. R. C. CROSS: Might I suggest if there is any difficulty in this matter that Mr. Turner should amend the title of his committee and call it a General Purposes Committee. If there is any doubt as to what the functions of a so-called executive committee would be we might call it the General Purposes Committee which could exist alongside the Council. It seems to me the essential point of Mr. Turner's proposal is that this committee should be an elected one; that is all the point in Mr. Turner's remarks.

Mr. C. C. TURNER: I would much prefer it to be called the General Purposes Committee.

Mr. H. DELACOMBE: I would point out, Sir, that the suggestion of Mr. Reid to the resolution or amendment that the Council should be elected would absolutely ruin all the traditions of this Society in my opinion. (Applause.) The Society was formed in 1866. It boasts of being the oldest Society in the world of its kind, and you can only make this Council a Council elected by the members by altering all your original ideas and constitution, which I think is a very false move

at this time, when the study of aviation and aeronautics is going very rapidly. I do not see how Mr. Reid's idea of electing new members to the Council through the members of the Society would be doing anything for the benefit of this Society. The Council as a scientific body should always remain as it is. But the members should elect with the sanction, and I should think the cordial co-operation, of the Council, an executive or General Purposes Committee, or whatever you like to call it, to carry on the plain drudgery of ordinary routine, choosing of grounds, etc., it should have nothing to do with the scientific side, which is absolutely separate. That members should elect this body seems to me the most natural thing, and it can be done very easily by the Council stating that they are quite willing for such a body to be elected, annually, biennially, or whenever you like, by the members. It would not be trespassing on the ground of the Council in any shape or way as the suggestion of Mr. Reid that the Council itself should be elected by the members certainly would do. Your original idea was that you were a self-elected Council. You started in 1866 as a self-elected Council. It is far too late in the day for the oldest Aëronautical Society in the world to alter its Articles of Association, if I may so put it. (Hear, hear.) At the same time everything has changed since this Society came into existence, and if you are going to keep your flag flying at the mast head, so that everybody recognises you, you have to move along with the times, and you have to do a lot more work outside scientific work than you have done in the past, otherwise you will have what Captain Marriott very truly said just now, other people jumping up and saying, if the Aëronautical Society won't do this, that and the other, we will do it and get the credit for it. I think the Aëronautical Society should do their best, and they can only do these things if they have a pretty quick-moving General Purposes Committee to help things along, and, as Colonel Fullerton said, the necessary means for bringing it along, because money is the mainstay of all progress. (Hear, hear.)

The PRESIDENT: The resolution has been proposed, seconded and spoken to, and an amendment has been proposed by Colonel Fullerton and seconded by Mr Bruce.

You have heard the Resolution and you have heard the Amendment, and it is my duty as chairman without any comments whatever to put the amendment first.

The amendment was submitted to the meeting and rejected. 9 voting for and 20 against.

The resolution was then submitted to the meeting and adopted. 21 voting for and 9 against.

GENERAL MEETING

The First Meeting of the Forty-Fifth Session of the Aëronautical Society of Great Britain was held at the Royal United Service Institution, Whitehall, London, S.W., on Friday, March 18, 1910, at 8.30 p.m.

The President, E. P. Frost, J.P., D.L., presided over a large attendance.

In opening the proceedings the President said: Ladies and Gentlemen, I am sure Mr. Headley requires no introduction. He is a most illustrious naturalist. He has written most interesting books, and we shall hear with delight what he has to say.

THE FLIGHT OF BIRDS

By F. W. HEADLEY

1.—RAPID MOVEMENT ESSENTIAL

The bird lifts and propels itself by means of levers, and for these levers a fulcrum has to be found on the yielding air. How is this possible? Newton's experiments led him to conclude that the resistance of the air increases as the square of the velocity, and when great speed is attained the increase is even more rapid. Here is the physical fact that makes flight possible. No cold-blooded animal capable only of short, rapid spurts could possibly fly. No halting to rest is possible. Effort must be uninterrmitting.

A bird's flight sometimes looks very leisurely, *e.g.*, the flight of a rook when he flaps comfortably along. But really the ends of his wings move very rapidly, and it is there mainly that the fulcra are.

This rapid movement is brought about by the attachment of the depressor muscle to the humerus quite close to the body. A lever of this kind aims at rapidity. When the near part of the wing is moved a little, the extremity moves much, but not so much as would appear when the wings of a bird almost meet beneath it, for this is not entirely due to the lowering of the wings, but partly to the raising of the bird's body.

2.—FRESH COLUMNS OF AIR

But rapidity of movement would be unavailing if the wings were upon the same column of air. There must be continual onward movement. A pigeon tied by a string fastened to his leg falls to the ground directly he reaches the limit of his tether. When birds are flying together in flocks, each is careful not to put himself exactly in a line behind any other. Were he to do so, not only would he be met by a backrush of air, but also he would find himself on columns of air that, owing to the wing beats of the bird in front, were in a state of dilapidation.

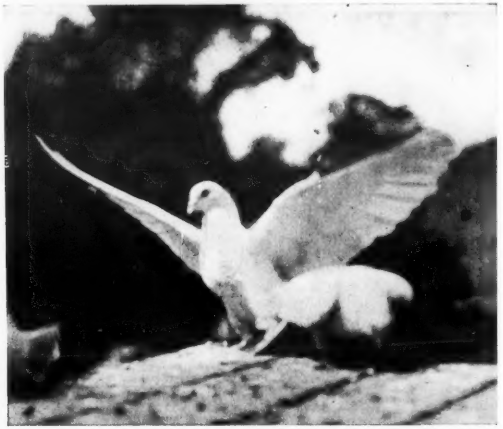
3.—LIFTING AND PROPELLING WORK OF WINGS

Obviously screws are out of the question in a machine which, being a live animal, cannot have one part separable from another. Levers are the only alternative, and they must be made not only to lift, but propel. For this purpose they must be inclined downwards and forwards. This may be effected in two ways. When the wing is extended the front margin may be lowered—the attachment of the muscles to the præaxial edge of the under face of the humerus tends to bring this about. But when a long and powerful stroke is taken, the wing moves forward as it descends and the required incline is obtained from the base to the extremity. And, of course, the two methods may be combined.

4.—THE PHASES OF THE WING-STROKE

In order to begin the down-stroke the bird raises its wing so that a line from the front margin to the hinder margin is parallel or nearly so to a line from the head to the tail. The wing then descends, pointing more and more forward as it goes and turning face downwards. When no further descent is possible the muscles relax, the front margin of the wing rises relatively to the back, and then the wind due to the bird's own velocity does most of the work of lifting the wing into position for the next down-stroke.

When the stroke is a short one, as it is with many birds in horizontal flight, there is very little forward movement, and the propulsion is due to a slight lowering of the front margin relatively to the back. A very slight incline from the hinder margin downward is sufficient, for the air offers but little resistance to the bird's onward progress. He fights mainly against gravitation.



Alighting
The protruding bastard wing is noticeable on left wing

The wings sometimes give unequal or dissimilar strokes

Gannets gliding
The one in background has wings partly flexed

The wings are being raised edgeways—the feathers slightly parted—to allow the passage of the air

The beginning of the up-stroke

Ready for the down-stroke

5.—FREEDOM OF MOVEMENT ON A FIRM PIVOT

The shoulder joint is remarkable for the great freedom of movement which it allows. At the same time the bones which meet there form a firm pivot. The coracoid is remarkably strong and points more outward in strong than in weak flyers.

6.—BREATHING DURING FLIGHT

It must be remembered that the bird is a flying machine which has not only to fly but to breathe. If respiration were effected by the moving forward of the breast-bone and the connected bones, the pivot on which the wing moves would not have the necessary stability. The method adopted during flight is, I believe, undoubtedly this; the lung chamber is enlarged by the raising of the backbone while the breastbone remains steady. When a bird is lying on its breast the hinder part of the back beneath which are the great air-sacs may actually be seen to rise. During flight the pressure of the wings at the shoulder must tend to check the movement of the breast. In fact, it is easier for the muscles to move the back.

7.—THE SPREADING AND FOLDING OF THE WING

The strong muscles which work the wings are massed upon the body: the wings themselves are light. Ligaments play an important part and among them the elastic ligament, through which the secondary wing feathers pass and which causes them to spread like a fan. The rapidity with which the wing is spread is remarkable, and no less the rapidity and the neatness with which it is folded. It does not in any way incommode the legs, being in this respect very unlike the wings of the Pterodactyl and the bat.

8.—DIFFERENT FORMS OF WING

The best flyers, if we except those which soar, have long narrow wings. When the wing moves rapidly forward and downward it is the front margin which does most of the work. This principle is known as the law of Avanzini and is easily explicable.

9.—STABILITY

When a bird is flying he has the advantage of having his centre of gravity low. The great depressor muscles which spring from the breastbone weigh very heavy. Indeed, in the pigeon they account for one-fifth, in some cases slightly over one-fifth, of the whole weight of the bird.

But, undoubtedly, a bird trusts more to perpetual, slight, voluntary adjustments than to automatic stability. Anyone who watches through a field-glass the tail of a lark as he ascends in a fair breeze, or, still more, anyone who watches a gull following a steamer without any wing-beats, must, I think, come to this conclusion. The perpetual slight movements of the gull's wings are easily visible. Even when a bird is standing on one leg he is, though very probably asleep, making continual small adjustments in order to maintain his balance.

Another important point remains to notice. The bird's body rocks when it flies with long, vigorous strokes. When the wings strain forward the centre of gravity moves forward slightly. When the wings move backward for another stroke the centre of gravity moves slightly in the same direction. This oscillation the man-made aeroplane is, of course, free from. But the bird has this great advantage, his nerves and his muscles obey with lightning speed an order from the brain. No aviator, however skilled, can possibly make his balancing apparatus work with such precision and so promptly. There seems more hope in the direction of automatic means of achieving stability. In a field where such wonders have already been accomplished there is good reason to hope that this outstanding problem also may be solved.

10.—STARTING

The small bird is, as a rule, a first-rate starter. Take the lark as an example. He jumps into the air and is off. The swift, on the other hand, cannot rise, as far as my observation goes, from among fairly short grass, possibly not from a smooth surface. All big birds rise with comparative difficulty. Helmholtz undertook to prove that a big flying bird was an impossibility since, when the linear dimensions are increased, the bulk and consequent weight will increase more rapidly. This is true, but the advantage the large wing has over the small wing is much greater than its superiority in area. But the great argument is derived from practical experiment. Mr. Maxim's aeroplane weighed 3½ tons, a monstrously big and heavy bird, and it actually rose in the air.

The difficulty big birds have in rising is very largely due to stiffness at the shoulder joint. The lark, in rising, gives his body a steep upward incline, but so turns his wing that it beats, not backward and forward, but up and down. A big bird, as I have found by observation on many, is too stiff at the shoulder. The up-and-down beat, if his body is set at a steep incline, is an impossibility for him.

11.—FAST FLIGHT AND SLOW FLIGHT

In fast flight the body is inclined at a small angle to the horizon. Consequently, the fast flyer meets with comparatively little resistance. Besides this, most of the resistance comes in the form of support. The wind—the wind due to the bird's own velocity—acts at right angles to the aeroplane presented by the bird. The line, therefore, that represents the force of the wind is nearly vertical. The slow flyer has to set himself at a greater angle to the horizon; consequently, not so large a proportion of the resistance of the air comes in the form of support. Professor Langley has shown that it requires less force to fly at a rate of 17 metres per second than at 14.*

12.—GLIDING

What has been said under the last head about resistance and support applies also to gliding flight. This point, in addition, is important. The front margin of the wing does the greater part of the work on the principle mentioned above. If, therefore, a bird wishes to maintain his level or glide upward he spreads his wings to their full extent. If he wishes to descend he partly flexes them.

13.—STEERING

The tail is used as a rudder to some extent, one side or the other being lowered, but it plays a much larger part in balancing. A rook that has lost his tail has no difficulty in steering. The bird steers mainly by throwing himself on one side—on his left if he wishes to go to the left, and *vice versâ*. I feel certain, too, that he often gives unequal strokes with his wings. It is very hard to detect this with the eye, and often photographs fail altogether to make it clear. But I have obtained a fair number of photographs of pigeons in which the wings are certainly not balanced.

14.—ALIGHTING

The way in which a bird alights is enough to make any aviator jealous. There is a total absence of jar. Every stitch of canvas is spread. Even the bastard wing is conspicuous. If there is a strong wind the bird will often descend with it, but, if so, he invariably turns when he nears his landing place and faces the wind, which checks and steadies him.

15.—WIND AND FLIGHT

(*a*) *Rising.*—All birds face the wind when they rise. If they did not the wind would, at the start, beat upon their backs and force them downward. But the

* Experiments in Aero-dynamics, p. 37

great advantage they gain results from the fact that wind increases in velocity with altitude. The increase is, of course, gradual and continuous, but for the sake of clearness we may divide the wind into layers. As a bird rises into a higher layer he has inertia, due to the fact that he was a moment ago part of the slower layer below. Having inertia he resists the wind instead of moving with it. He is like a kite held by a string, and, consequently, is lifted.

(*b*) *Gliding on an Upward Current.*—Everyone has observed gulls following a steamer without any flapping of their wings. They hang over the stern and hold their wings outstretched. When they do this there is always a head wind or wind making a small angle with the vessel as it blows over it. Striking against the ship the wind gets an upward incline, and on this up-current the gull poises. Now, if he inclines his body slightly downwards, so that the line at right angles to his body that represents the force of the wind points slightly forwards, then he will be not only sustained, but propelled.

(*c*) *Soaring.*—Some mathematicians have held that soaring is possible in a horizontal wind if only there is inequality of velocity, so that the bird can pass from a slow current into a rapid one and so have the advantage of inertia, of resistance to the wind, and be lifted. But as he circles round, the wind must strike upon his back during part of the gyration. When he has just changed direction so as to fly with the wind instead of against it, the force acting upon his back will drive him down instead of lifting him till he moves at a pace equal to that of the wind, and then it will give him no support. We may feel sure, therefore, that birds soar only in a wind that has an upward incline. This explains the fact that birds are so often seen soaring in a mountainous country. They may often, also, be observed soaring over level plains, but in this case only when there is a bright sun. The ground becomes heated, with the result that the air rises. It collects in columns and the bird, no doubt, circles in order to keep within the ascending current and out of the neighbouring descending currents. When there is a very long stretch of ascending air birds will sometimes advance for great distances without circling. I have seen eagles fly quite a mile and a half straight ahead without a single flap of their wings. Summing up, I may say that the wind is the friend of the bird. To the aviator, I fear, it is an enemy to be conquered.

(*d*) *Flight with the Wind.*—But in one case the wind is an enemy to the bird also. When it blows a hurricane or something approaching to it I think there is pretty good evidence that the bird cannot fly *with* it. When he is starting to fly with it and is as yet moving slowly, it will impinge upon his back and force him down. But when he has once got up pace it is not so easy to see how the wind interferes with him. Undoubtedly a more moderate tail wind is a help and not a hindrance, and it is in a tail wind that the best times are made by homing pigeons. I think this may be the explanation: the more rapid the wind, the greater its variation in velocity. Let us imagine the bird having the velocity of the wind + 25 miles per hour due to his own exertions. Yet a sudden unusually violent gust might make it difficult for him to draw back his wings for another stroke.

16.—THE REPAIR OF THE MACHINE

The flying machine I have been describing consists largely of feathers. A feather is light and it is elastic. Any form of living tissue would be heavier and less suitable. But a feather, being a dead thing, cannot last for very long. It must be shed and replaced; there must be an annual moult. And in most birds this is so managed, by shedding the flight feathers gradually and in pairs, that they are at no time incapacitated for flight. The bird, in fact, is more than a flying machine. He is a self-repairing flying machine, and since he reproduces his kind he is, in a sense, imperishable.

17.—CONCLUSION

In conclusion I would say that, in my opinion, it would be unwise to try to imitate birds too closely in some important points. It may be because I hold a

brief for birds that I maintain strongly the view that an aviator can never rival them in skill, in the skilful coping with gusts and eddies. He must trust more than the bird to automatic machinery to give him stability, and he can never soar and utilise up-currents as the bird can, since it is impossible for him with so nice a touch to feel the pulse of the wind. On the other hand, it is impossible to doubt that, now that many of the best brains are applying themselves to the problem of flight, improvements in design and in the details of machinery will follow fast upon one another, so that aviation may or, rather, must have a great future before it.

DISCUSSION

Mr. S. F. Cody: Mr. Chairman, Ladies and Gentlemen, I must confess that this lecture has had more charm for me than anything I have ever heard in any of the Society's meetings. I have made a strict study of the bird, and this is the only lecture that has taught me anything in the study of mechanical flight, although the bird is not mechanical. I do not happen to know exactly which bird produces the most power, but one very distinct argument that I have always had and believed in was that the wings in the bird gliding, in the best gliders such as the turkey, buzzard and the sea gull—I have not seen the albatross or the kite much—slant down from the shoulder and not up straight. I should like to know from the lecturer if he found that the case, as he has made a very strict study of the subject, perhaps more so than I did. I take my cue from the duck, as the bird that carries the heaviest load, and the partridge, and all their glides and landings are invariably with the wings very much down. I should like to know what his opinion is about my argument, and to know if he agrees with me in that respect, and to know which bird has the least power and glides the best. He evidently has studied the sinews and muscles of the bird very much, although he did not give us any advice as to what bird to imitate with a fixed plane to glide well, as gliding is the most important thing in mechanical flight that we can work on. A mechanical machine that is not a good glider is a dangerous one, and I think we should follow that very strictly. The gliding qualities of machines should be considered before we risk our lives in them.

Mr. Holroyd Smith: We ought to be very much obliged to the lecturer for the very interesting set of photographs he has displayed upon the screen. There is one point I am very pleased to hear from him, namely, that he confirms an observation by Leonardo da Vinci, that I heartily agree with, and have heard of no other confirmation, namely, the unequal vibration of birds' wings. Leonardo da Vinci says that birds will flap one wing and not flap the other. I gather from the lecturer that the bird can flap one wing unequally with the other. In a Society like this I cannot listen to a lecture, such as we have had to-night, and remain quite passive, when I feel that the lecturer has propounded one or two rather erroneous deductions from his observations of birds. I cannot deal with all of them, but I shall deal with one or two. In the first place we will take that question of the separated feathers on the tip of a bird's wing. He told us to my surprise that his deduction from that was that the separated tip of the feathers aided the stability of the bird. Surely anyone who has observed and studied the operation of birds will see that it has a far different function and use for the separation of those feathers than the question of stability. I do not know whether time will permit me to enter into any strong controversy on that point, but I ventured to deal with that very subject when I was lecturing on birds before another society. There is a very different function for those separated feathers. They have nothing to do with the question of stability as the primary object, but they have principally to do with the question of propulsion, if the form and shape and operation of the feathers on the wing of the bird are really carefully understood and analysed. Another question that I dispute with the lecturer to-night is that he attributed the hovering of the kestrel to those feathers. The kestrel hawk is a splendid bird and a beautiful hoverer, but I do not think the way in

which it was described to-night was in any way a correct explanation of that wonderful and marvellous function. Another point I must dispute with the lecturer is this: he was very strong indeed in an assertion, almost a positive assertion, that it was the front edge of the bird's wing that did all the work. To say the front edge of the bird's wing is a highly important part of the bird's wing is perfectly correct, but to say that it does all the work, and then follow on with the rather curious argument about the result of the air that struck the front of the wing and prevented any pressure going upon the latter and further curve portion of the wing, was so rudimentary a deduction that I am a little bit surprised that it should be brought forward by anyone who has made such a most careful study, and has had the opportunity of taking all those photographs. Another function, and a rather important one, that the lecturer left out altogether in his explanation of the action of the bird's wing was the fact of the propelling power of that wing. He seemed to tell us it was due to the wing descending on a slope; he did not give us the explanation which I think those of us who have really examined the wing know to be the fact: that it is due to the flexibility of the rear portion of the wing, and further that that flexibility of the rear portion of the wing is just as effective in propulsion in the up-stroke as it is for the down-stroke of the wing. Now, Mr. President, I do not feel inclined to go in for a strong sort of opposition argument against such a careful observer as the lecturer to-night, but I can hardly keep silent and allow those few points I have mentioned to be taken, by those of you who perhaps have not had the opportunity of studying the subject, as the correct solution of these matters. I am afraid that some of us get a little bit into a vein or a routine of observation without keeping our minds sufficiently open before we arrive at deductions, before the deduction is emphatic. It means that we are very careful to be right in our observations, in fact, I take the liberty to mention again the definition that I have before ventured to give of Science, namely, that Science is a logical deduction from the intelligent observation of accumulated facts. We have had a most interesting lot of pictures shown here to-night. The one that has attracted me most was a particular one of the gannet and that turkey buzzard. I thank the lecturer for the wonderful lot of photographs that he has given us, and I must apologise if I have been a little too drastic in my remarks.

Colonel H. E. TYLER: I think after this very interesting lecture that it is hardly right for anyone who has really made much observation of birds in flying not to make a few remarks to supplement the lecturer if he will permit it. Twice on board ship I have seen the tern, which is a small bird with very long wings, unable to rise. I attributed it to the very long wings beating against the ground. I do not know whether it is the right reason or not, but on putting it on a slight elevation I have seen the tern fly off at once. I had a very interesting look at some gulls and jackdaws on Beachy Head two years ago. The case of an up-current in the air was very well provided by a wind on Beachy Head that naturally is deflected upwards by the Head. There was not one gull that swung the wings at all, not one of them made a stroke, they all soared about where they liked, and there was not a single jackdaw that could soar, owing, no doubt to the larger wings of the gulls. As regards the uneven working of the wings I have noticed it thousands of times looking over the stern of a ship and throwing a bit of bread into the water; in turning to get the bread before another gull they almost break their wings, and I have been told that skippers of ships knew of cases where the wings were broken. Then there is one point I do not think I have ever heard made on the subject of birds—I think there is not much doubt that it is the case—and that is the remarkable assistance that feathers give to flight by their resistance to motion in the wrong direction. There is almost a positive assistance to flight, I think, in the fact that you really cannot force a feather against a wind without considerable resistance. There was only one point on which I felt a little bit inclined to differ from the lecturer, and that is, where he was telling us that the front part of the wings of birds gave the larger part of the

assistance to flight. I have not the slightest inclination to question that, but he illustrated it by the sailing of boats. I have done a good deal of boat sailing in my life, and I have generally found a good broad sail do very much more than two narrow ones when sailing close to the wind, but when you are doing your best against the wind in a small or big boat, you will find the luff of your sail shake, that is to say, the work of the sail is done a little further aft. It is not in the least incompatible with the lecturer's statements with regard to the birds, but I do not think it is quite right about boats. I think the greater part of the work is done nearer the centre of the sailing boat, and the reason is there is a great resistance to the movements of the boat in the water, and the bird does not have that same resistance in the air.

Major BADEN-POWELL: I should like tosay a word if I may to back up the lecturer in his idea of the separation of the feathers adding to the stability of the machine. I have often made little experiments with small models, and I have distinctly found that if a surface be cut up into several portions the stability is undoubtedly increased. I never before heard the suggestion made that the spread ing out of the feathers at the end of a bird's wing added to the stability, but I cannot help thinking there must be a great deal in it. I cannot agree with Mr. Holroyd Smith when he says that is to add to the propulsion of the bird, because, after all, these separated feathers are chiefly observed when a bird is soaring and not moving its wings at all. Therefore it is not wishing to propel itself along. There is one other little point that I was going to bring forward. First of all, I should like to say a good deal more about Dagenham, but I did not have the opportunity. If the members of the Society had come down to the ground at Dagenham one day they would have seen one interesting experiment made there, an experiment which you do not see every day, and yet it was almost every day to be seen there, and that was to watch a hawk hovering. We had a tame hawk there; at least, when I say a tame hawk, it was a hawk that apparently attached himself to the Society, and used to come there every day to give us lessons in flight. We used to watch him very carefully through field glasses. I have never had a opportunity before of watching a bird so carefully and steadily for many minutes on hand. Now, we know the well-known illustration of the hawk which forms the Crest of this Society, of which Mr. Holroyd Smith has recently given us a picture, and we know the way it is drawn. It is supposed to be a kestrel hovering, and its tail is spread out in an almost vertical position. Well, now, what we particularly noticed, and we often discussed it, was that this bird hovering over the grounds at Dagenham always had his tail perfectly horizontal. He faced towards the wind, moved his wings rapidly but always in a perfectly horizontal position, and I think that is a point which is worth noticing, because it seems to be often considered that a bird in hovering holds its tail vertically downwards. (Applause.)

A VISITOR: Mr. Chairman, I do not wish to allude to the various arguments advanced as to the various movements of the wings, but I should like to say one or two words with regard to the hovering question. The lecturer, in giving us that beautiful photograph of the bird hovering, mentioned that the wind had an upward pressure and to some extent came upon the back of the bird. I am inclined to think after a considerable amount of study of the question, that that is hardly correct, for this reason, that very often a bird will hover when there is no wind at all and quite away from any tendency of pressure of wind, and I think that the reason why the bird hovers is partly owing to the cutting edge; the cutting edge causes the wind to be divided so that you get a compression of the wind going underneath the wing, and you also have a rarefraction of the air above the wing, with the result that you have much less pressure above the wing, and you have a little greater pressure under the wing. And that, taken in conjunction with the large bows of the bird, and with the high temperature which is about 102° Fahr., will explain to a great extent how it is a bird of great weight

is able to go a great distance apparently without any movement of the wing whatever. I can hardly imagine any cause other than the one I have just given. The other parts of the lecture are very interesting, and show that we have the natural movements of the bird, but when we come to deal with aeroplanes generally we have to take into consideration the artificial movement, and that is where, if we can bring in rarefraction on the upper surface and compression under the surface, lies the secret in future gliding.

Mr. B. G. COOPER: On the question of the movable feathers upon expanded effort, the late Mr. Muybridge, who made a great study of animal locomotion, proved by means of photographs that whereas these feathers on the down-stroke came down like that (illustrating), on the up-stroke they came up edgeways. I do not think the lecturer mentioned that.

Mr. HEADLEY: It certainly is so.

Mr. F. L. RAWSON: I would like to ask the lecturer whether he has had any experience with regard to a matter that interested me at one time. I was retained by the *Daily Mail* to make an examination for them into the results obtained by Hart who claimed to be able to move matter with his mind.* I need not trouble you with any of the results except one, and that was this. I pivoted a ball on a slight rod so that the ball could move around in this way (illustrating) and one of the things he was able to do was to blow at the rod on the ball and make the ball come right up into the wind. When I looked into that I found that if you have the wind with a pair of bellows slightly off the centre and blow right on the ball, the effect of it was to bring the ball right into the wind, the harder you blew the more it came up, and if you get a good breeze you find the ball working like that, continually being caught as you get the breeze off the side, and when you get it on the spot it comes right up again with tremendous force and power in that wind, showing there is something there we do not quite get hold of. And there may be some action there in regard to birds that we have not quite got at. The second thing I would like to mention is, although we are unable to practise flying we are able to practise movements in liquid, and that is in swimming, and there one often gets the same results as we see with birds, especially in the midst of currents. When I saw that bird in those pictures in the extraordinary position, the first one, it put into my mind the position where one hand has to go one way, a leg the other way, and another hand another way to keep oneself in position in the mixture of currents. I do not know whether you have noticed the tremendous effect if you get two currents going at right angles to each other, and it may be the bird is just caught in the middle of a current in that way.

Colonel FULLERTON: I am afraid we have rather bombarded Mr. Headley with questions to-night, but as I am on the Bird Construction Committee, and am very keen on this subject, there are one or two things I should like very much to have his opinion about. First of all there is the question of the action of the outer part of the wing in balancing. I should like to hear his opinion as to whether the bird does really use the outer part to balance himself. The reason I ask is that I was talking to M. Blériot's representative the other day at the show, and he told me that M. Blériot now never uses his warping apparatus at all, that he does the whole of his movements by shifting his weight right and left, and when I asked what was the good of the warping apparatus, he said it was easier for beginners to learn their balance that way. That is what was told me and I thought it was an interesting point. There are one of two other matters I should like to ask about. One is a question about the number of beats made by a bird per second. It is a very difficult one, as the lecturer says in his book and I hope that he will be able to give us some really realistic information on this

* See letter in Correspondence

subject later on. I have a number of details of the sort, but I am not quite clear whether they represent the beats in average flight, or best flight, or what they represent. For instance, you know in Marey's experiments, are the numbers given for average flight, or the best flight or what? Then as regards the power of the breast muscles, the only experiment that I have been able to get hold of is Dr. Smyth's. He electrified a small pigeon and made it, by flapping its wings, lift a certain weight and found it did 5 foot-pounds of work per second, that is to say, the muscle material of that particular bird was capable of doing about 5 foot-pounds of work per second. That appears to be rather low, and I should like to know if there is any more information available. Then as regards the velocity of the birds, the lecturer has given in his book a great number of flight velocities, including one of a swallow travelling at the rate of 106 miles per hour. Such a rate appears to me incredible, especially if it took place in still air. Possibly the reduction of wing surface may increase the speed, as birds with reduced wing area must go a faster pace to keep themselves up. It seemed to me that these enormous speeds might be due in some way to that, and I should rather like to know your opinion on the subject.

Mr. HEADLEY: I am sorry to say that I have not a pencil upon me so I shall have to trust to memory in answering these questions, and I hope that those who have asked them will remind me of any I do not say anything about. Many of them are, of course, uncommonly difficult. With regard to gliding, my experience is that the best gliders glide with their wings fully stretched out certainly, but I have not made observations on all birds—that is obvious. The gannet, which is a first-rate glider, certainly has his wings at full stretch; directly the bird wants to descend, as he glides, he partly flexes his wings. Then with regard to the question of the propulsion, I may say I think my view at any rate has the advantage of being supported by Professor Marey. He says that the front margin of the wing is lowered during flight, as compared with the back margin and the propulsion is due to that. He certainly states that, and the only point I added to that is that when very vigorous strokes are taken it is the tip that is lowered below the base, and I think the propulsion is due to that. The principle is exactly the same—the front part being lower than the back, but in hurried flight it is the tip that is lower than the base—the principle is exactly the same. With regard to the big flight feathers by separation giving stability; that was only a suggestion which after all I am glad to have supported by a high authority. I do not wish to lay down the law on that principle, but now I am beginning to hold it more strongly since I see it so well supported.

A MEMBER: It has often been found that it is the case—as a matter of fact you get stability by compression of parts.

Mr. HEADLEY: I do not see how these big flight feathers can propel the bird when he is soaring and not using his wings at all. The feathers are perfectly dead things, and a bird cannot move them like that. Some people have said they have seen them moving like that, and the bird when propelling was moving individual feathers. An elementary acquaintance with the anatomy of the bird should have knocked that idea on the head. It is quite true that feathers do seem to move; some will stand up more than others, but I think that is because they are more flexible, and, the wind acting on them, the upward current that I believe in acts upon and bends some up more than others. There are no muscles to move an individual feather, and even if there were, they are very weak muscles near the end of the wing, and in consequence I do not think the bird can be propelled by that means. With regard to the up-current—this strengthened my view very much that the bird does depend on that. I was in the Delta in Egypt, this last spring, and there were a number of kites there soaring in the bright sunshine. Suddenly a cloud came over the scene, and very soon these birds all descended—that is a thing you may notice every now and then, directly the ground ceases to be heated and the up-current comes to an end, the

soaring is over. This matter of the front margin of the wing doing the main work, I am glad to find that is borne out. When I find people agree with me I naturally attribute more weight to their views than to the views of those who I find entirely disagree with me. I am rather like the rest of the human race perhaps in that, but I think we may look upon Avanzini's law as sound. This cushion of air, the air that struck against the front margin of the wing, rushes past underneath it and keeps off the air that would naturally come to the back part. You find the same thing when the wind is blowing at right angles to a wall. If you wait close to a wall the wind coming off the wall prevents your feeling the wind blowing towards it—if you keep close to it. Then in regard to the narrowness of the wing it certainly is not so important to a soaring bird. It is only when the wing is cutting through the air both forward and downward at the same, it is then the narrow wing is most efficient. Of course, birds are great experimenters in this, and the result of their millions of years experiments has been that the best of them have come to the conclusion, if we may put it so, that the narrow wing is best, and that seems to me equal to saying it is the front margin which does the work. Is not the air striking the ball similar to putting a cut on a golf ball and it spins round?

Mr. B. G. COOPER: It is fixed at the end of the rod so that it can only curve at the centre. A piece of stick is pivoted here so that it can turn; if you take a celluloid ball and fasten it on to the end and blow it at an angle, it will come up across the wind.

Mr. HEADLEY: I thought the ball had freedom.

Mr. B. G. COOPER: No, the ball is fixed.

Mr. CODY: The question of the aft part of the wing not doing work ought to be clearly destroyed when you watch a Farman aeroplane balance itself by flexing only the very aft edge of a portion of the wing—it gains an extra lift by flexing with a very slight body. It may come down an inch on that side and allow that side to go up an inch, and the machine will commence to right itself at once. So the aft of an aeroplane, that is a mechanically driven aeroplane, must work. I think the aft of the wing of a bird does a little work, but there is no question about the front doing the most work, because we know that we have to put all the weight of our machines near the front edge, in fact, the machine would balance sitting on the front rail. Mine, for instance, balances absolutely on the front rail—the weight of the machine is farther front than the Wright machine because my curve is like the Blériot—more deep on the front edge. Speaking of M. Blériot's advice to Colonel Fullerton, allow me to say, if my opinion is worth anything to the Society, don't go up on an aeroplane hoping to balance by shifting your weight, because I am willing to demonstrate this by taking a man four feet away from me on one side and nobody on the other and take him up and fly as straight as if I was flying wholly by myself. I have flown to one end of the plain, broken the two uprights of my machine in a bad landing, started up and flown back, and didn't know they were broken till I got back. It shows you that the loss of lift on one side is not going to hinder you very much. I have a very powerful balance in the double elevators, but that the man can balance the aeroplane by leaning his weight over to one side is a mistake I am pretty sure. When a Blériot machine can go up and turn sharp without his balancing wings the same way the engine is running, then I will believe it. But remember, the Blériot machine turns to the left while it flies—in fact, I have never seen one turn to the right sharp in the air. If they attempt to turn a short corner this way, and the engine is running that way they are going to come to grief, if they haven't some twisting or balancing device with them. I think the double screw aeroplane is the only one that can turn in either direction with ease—that is, turned quickly. I don't know much about the Farman, but I know the Wright Brothers turn in both directions very quickly.

The PRESIDENT: We have had some very beautiful illustrations of natural flight, and I think photography in most instances tells the truth—sometimes painfully so—but in this, notably in flying birds, I think it very often does not give us a correct conception of wing movement, that is to say, the two wings at the same moment in many degrees of flight. But I am sure we are all agreed this evening in giving to Mr. Headley our heartiest thanks for the most interesting and instructive lecture this evening. (Applause.)

Mr. HEADLEY: I must thank you very much indeed for the very kind hearing given to me, and for the supplementary remarks that have been made. With regard to anyone with whom I differ, I may say that I have not tied up my bundle of opinions absolutely, but that I am wanting to learn further as far as I can. I look to the birds as my chief instructors, but I also am very glad to pick up anything I can from any aviator. I look at any man who has flown with the utmost respect, and I am also glad to hear anything from any brother observer of birds, and if any of my theories are knocked over I shall take it quite in good part. I do not wish to lay down the law on any point, but on some points I give up what conclusions I have come to, I must own, with some reluctance. I thank you very much once more. (Applause.)

A MECHANICAL ANALOGY OF THE INSTABILITY OF A KITE

By PROFESSOR G. H. BRYAN, Sc.D., F.R.S.

The following method, while not intending to be anywhere near a correct solution, will nevertheless show that the instability of a kite in a high wind is not necessarily inconsistent with the assumption of the simplest possible law of air-resistance and a uniform horizontal wind velocity. Assume, in fact, the law of resistance $R = k S V^2 \sin \theta$, where V is the wind velocity and θ the angle of attack; assume, further, that variations in the position of the centre of pressure when θ varies are neglected, and that the kite string makes a constant angle α with the normal to the kite face. In other words, consider a mathematical problem based on the simplest and most elementary assumptions.

To construct a force-diagram, draw O A vertically downwards representing W, the weight of the kite, and on O A describe a segment of a circle containing an angle α. Draw O B horizontal and equal to kSV^2, and on O B as diameter describe a circle cutting the former circle in P. Then O P A is a triangle of forces for the kite, for $OP = kSV^2 \sin AOP$, so that if $AOP = \theta$, P O represents R and A P evidently represents the tension T of the string. (Fig. 2.)

If the kite drops, so that the inclination of its face to the horizon increases, R will now be represented by a new radius vector P′O; O A will still represent W, T will be represented by some line M P′, where $OP'M = \alpha$, and if A M be drawn perpendicular to M P′, there will be a resultant unbalanced force M A, tending to move the kite in a direction perpendicular to the string, and thus to bring it back to its former position. In the case shown in the figure, the unbalanced force tends to restore equilibrium and the kite is therefore statically stable. If V be increased, the diameter O B of the semicircle is increased, P approaches A, P O and P A become more nearly vertical, and hence the kite rises. *Statical* instability is impossible because if the restoring force M A were

zero (as would occur if the limit of stability were reached), the points O A P P′ would have to be on a circle which would therefore cut the circle O P B in three points.

Next consider the effect of imparting to the kite a *small velocity dv*, perpendicular to the kite string (Fig. 1). If the resulting increase of normal pressure dR is positive, it is evident that dR tends to retard the motion; if dR is negative, it tends to accelerate the motion. The former result suggests stability, the latter instability. Now draw the curve $r^2 \sin \theta$ = constant, taking the initial line parallel to the plane of the kite, and draw the radius vector C Q horizontal. Every velocity of the wind relative to the kite which is represented by a radius vector of this curve gives a constant normal pressure R on the kite. If, then, a

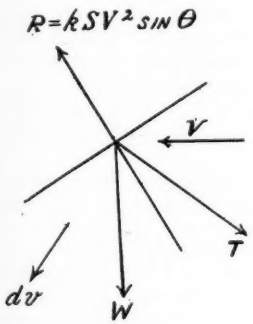


Fig. 1

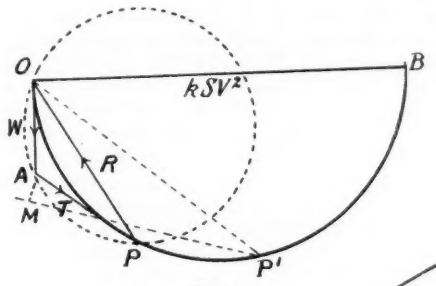


Fig. 2

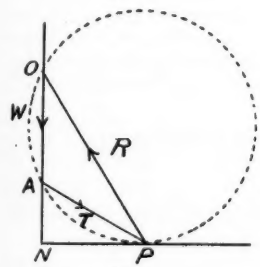


Fig. 4

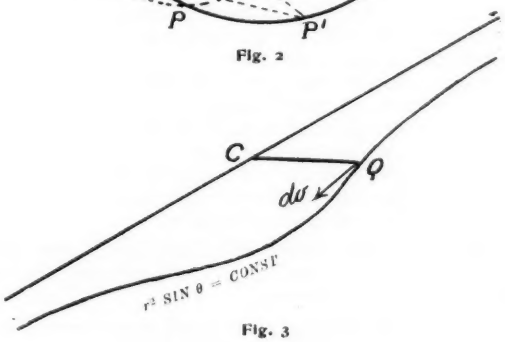


Fig. 3

line drawn from Q to represent the added velocity, *dv*, falls *within* the curve as shown, the value of R is decreased, corresponding to instability, if it falls *without*, we may expect stability. We may thus expect instability if the direction of the kite string is at a greater angle to the horizon than the normal to the curve at the point Q. It is easy to deduce the condition for stability $\tan \phi > 2 \tan \theta$, where ϕ is the inclination of the kite string to the vertical; and conversely the condition assumed to indicate instability is only satisfied if $\tan \phi < 2 \tan \theta$.

In Fig. 2, suppose a line P N drawn perpendicular to O A, and shown in detail in Fig. 4. Then $PAN = \phi$, $PON = \theta$ Hence, for the above kind of *instability* we must have $ON < 2AN$. The limiting case will occur where the circle through O A P touches P N, and the angle O P A is then the limiting value of α for which instability can occur. The angle between the string and the normal to the kite-face must therefore be less than the corresponding calculated value, which I find to be 19° 28′, in order that this instability may be possible. I find

moreover, that the limiting inclination of the kite to the horizon is then 35° 16′, and of the string to the vertical 54° 44′ and the resultant wind-pressure on the kite works out at 2.45 times the weight.

If, instead of finding the least value of the angle which the string makes with the normal to the kite, we require to find the least wind velocity for which any kite can be unstable, we must make the circle on O B as diameter, instead of the circle O A P touch the line P N where, as in Fig. 4, O N = 2 A N. In this limiting case we get O B = 2 O N = 4 O A, or $k V^2 = 4 W$; the kite makes an angle of 45° with the horizon, the resultant wind pressure works out at 2.83 times the weight, and the string makes 26° 34′ with the horizon; the angle the string makes with the normal to the kite being 18° 26′ or 1° 2′ less than in the previous case, that is practically the same.

If the wind velocity is small at first and gradually increases, the semicircle on O B will increase in size, and its intersection with the circle O A P will at first fall and then rise. This indicates that keeping the inclination of the string to the normal to the kite constant and supposing it to be less than 19° 28′ if the wind velocity increases, the kite will at first become unstable and then become stable, in other words, that there exists a maximum as well as a minimum limit of wind velocity consistent with instability.

If, instead of the sine law, we assume any other law of resistance, it would be necessary to substitute suitable corresponding graphs for the semicircle of Fig. 2 and curve of Fig 3. If we try to find a general solution of the problem in which displacements of the centre of pressure are taken into account, as well as the mode of attachment of the kite to the cord, and the rotatory inertia of the kite, we get a biquadratic equation, and while it is doubtless possible to obtain approximate solutions, as in the corresponding problem of the aeroplane, it hardly appears desirable to go to the same amount of trouble in this case, especially as at the end it would still remain to introduce modifications taking account of the weight of the string, its inertia and the wind pressure acting on it. Oscillations would thus be set up in the string as well as in the kite and the problem would be far too complicated to lead to any simple definite results. The only conclusions I would therefore suggest are the following:

1. The rising and falling of a kite may, under certain conditions, be due to dynamical instability, and may not necessarily be entirely a consequence of fluctuations in wind velocity.

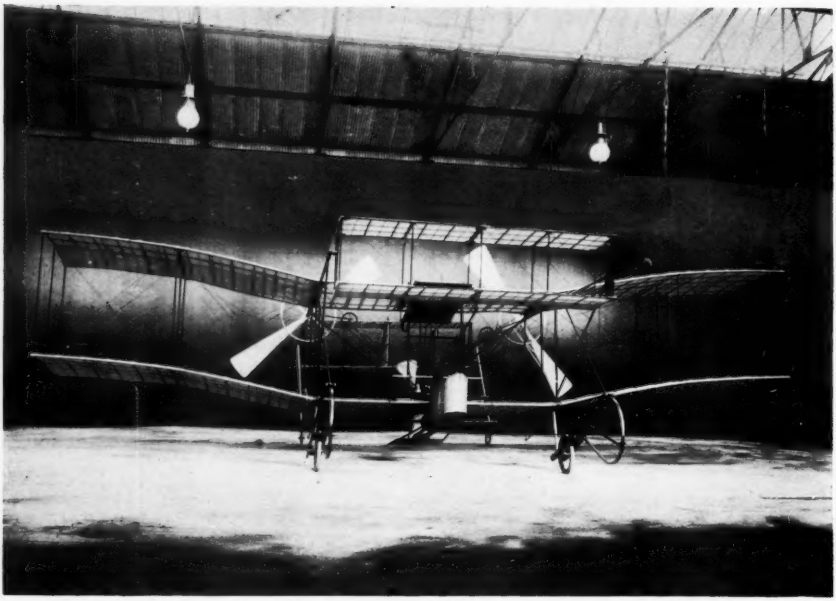
2. That a comparison of theory and experiment would be of much interest, especially in regard to the following points:

   (*a*) The extent of agreement or divergence between the actual observed conditions of instability and the formula $\tan \phi < 2 \tan \theta$.

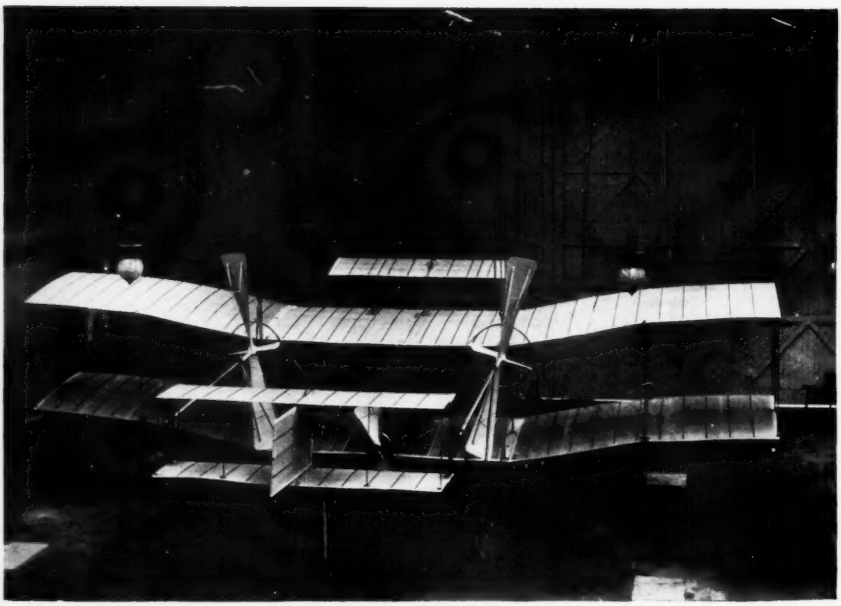
   (*b*) The existence or non-existence of superior and inferior limits of wind velocity consistent with instability.

3. The extent to which tangential resistances modify the conclusions seems worth looking into. (To take account of these the preceding geometrical constructions would require suitable modification and the treatment would be less elementary.)

Postscript.—**The "instability" indicated by the preceding conditions depends on the fact that, while, if the kite receives a small *displacement* the forces tend to restore equilibrium, if it has a small *velocity* imparted to it perpendicular to the string the forces tend to accelerate it. The equations of motion of a particle under an *acceleration* towards a fixed point proportional to the distance and a *retardation* proportional to the velocity represent *damped* oscillations; if, however, as here assumed, the latter retardation is replaced by an acceleration the oscillations instead of being damped will continually increase in amplitude. The result will be to make the kite bob up and down. If the accelerations always remained proportional to the displacement and velocity the oscillations would increase indefinitely in amplitude. In practice they will certainly increase until the "theory of small oscillations" becomes inapplicable.**



Sir Hiram Maxim's Biplane: Front View



Sir Hiram Maxim's Biplane: Rear View

MEMBERS MACHINES

No. 1:- SIR HIRAM MAXIM'S BIPLANE

The machine is a biplane with the main planes in three sections. The outer sections are set at a dihedral angle, the outer edges being raised 2 ft. above the inner edges. The middle sections carry the engine, propellers, and other machinery. The engine weighs, with all accessories, 220 lbs., and has given off 87 B.H.P. with a petrol consumption of 0.6 lbs. per B.H.P. per hour. The spread of the wings is 44 ft., and the total length of the machine fore and aft is 35 ft. over all. The main planes are 6 ft. 6 in. wide, and are placed 6 ft. 6 in. apart. The machine is provided with cross-connected fore and aft biplane elevators 13 ft. 6 in. long × 3 ft. wide and 3 ft. 5 in. apart. The pivot centres of the elevators are 28 ft. 8 in. apart.

Three propellers are provided, two being 11 ft. $3\frac{3}{4}$ in. diameter, and one 5 ft. in diameter. The smallest is mounted on the crankshaft and acts as a fly-wheel to the engine which is designed to run at 1,350 revs. per min. The large propellers are geared down and run in opposite directions, the weights and velocities being so designed that gyroscopic action is neutralised. The theoretical speed swept out by all the propellers is the same, thus giving all the propellers the same slip.

The camber and thickness of the planes are considerably less than in the French machines, being $2\frac{1}{4}$ in. and $1\frac{3}{4}$ in. respectively. The shape and dimensions of the planes have been derived as the result of experiment, it being found that a plane should be as *thin* as possible and that its entering edge should be set, as in the case of all turbines, so as to engage the fluid with the *minimum of shock*.

The engine abounds in novel features. Thus, the lubricating pump is provided with a reciprocating piston, which is forced up by a cam driven from the engine through a train of gear wheels. When the cam releases the piston it is forced down by a strong spring, thus giving an intermittent flood lubrication to every part of the engine.

The carburetter is of the non-float type. It consists of two concentric tubes, the inner one of which is perforated. Inside the inner tube is slidably mounted a piston consisting of two discs spaced apart by a small perforated tube.

This piston corresponds with the float of the ordinary carburetter, and carries the float needle, which is so arranged that the higher the float lifts the greater is the opening to petrol. Thus, when the engine is running slowly, all the air is drawn through the spray chamber, but when it is running fast, and a good mixture is easily obtained, then some of the ports are directly opened to the air. This carburetter makes the engine very elastic and steady.

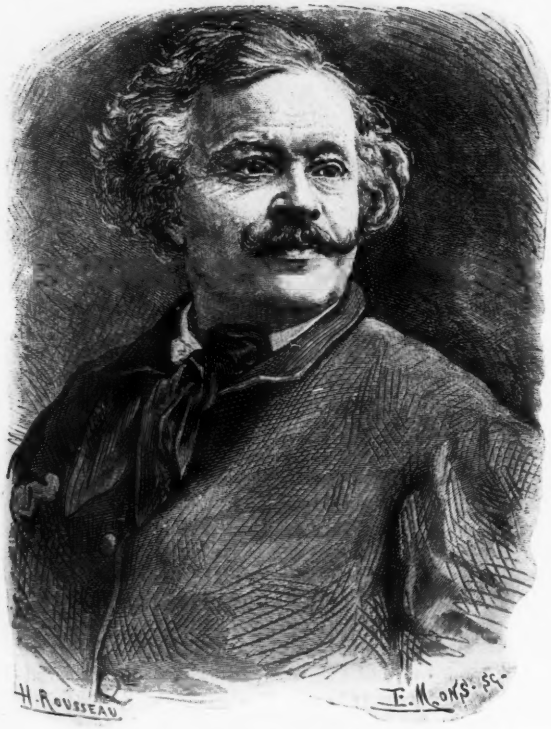
The machine has been built at the Gun Works, Crayford, and in the grounds adjoining, a tarred sand circular track, 25 ft. wide by 2,200 ft. circumference, has been prepared. A steel mast has been erected in the centre. This track is for the purpose of testing the engine and propellers under running conditions. Thus, the engine and the propellers may be mounted on a carriage provided with pneumatic tyres. This carriage may be connected to the tower by means of a small rope so as to counteract the centrifugal force. A most efficient and simple means for testing is thereby obtained.

When the propellers and engine of this machine are thoroughly tested, it is proposed to take the machine to one of the regulation flying grounds for further tests. The future of this machine will doubtless be watched with keen interest by all students of aeronautics.

A. P. THURSTON

NOTES

The Death of Nadar—M. Félix Tournachon—better known as Nadar—whose death occurred at Paris on March 20, was born in that city on April 5, 1820. At the age of 36 he took out the first patents for aerostatic photography in France and abroad, and in 1858, while above the valley of the Bievre, secured the first photograph (a positive on glass) ever taken from a balloon. Later he took up aeronautics, declaring himself in favour of the helicoptere, and built an immense balloon, the "Géant," the exhibition of which, he hoped, would bring in enough money to form the capital of an association to carry out his ideas. This association was called the International Society for the Encouragement of Aerial Navigation by Apparatus heavier than the Air. The "Géant," which was 120 feet in height, and of 210,000 c.f. capacity, failed to realise these hopes, although it undertook two



Felix Tournachon Nadar

journeys in October, 1863, from Paris, also making ascents from the Hague, from Brussels and from Lyons, and was exhibited at the Crystal Palace, Sydenham, in 1864.

Nadar, besides being the author of several novels, published "Les Mémoires du Géant," and "Droit au Vol" and founded in 1863, a short lived periodical *L'Aéronaute*. "Nadar's ascents," wrote Glaisher, "had not the remotest connection with science, although he claimed that they had; nor was his knowledge, as shown in his writings, sufficient to have enabled him to have advanced it in any way." This opinion was perhaps rather severe on one who had spent a great deal of time, money and enthusiasm in the cause, and who was the Father of Aerial Photography. If he cannot claim homage for his science, he at least commands for himself our affection and our respect.

Time Recorder for Flying-Machines—The following interesting paragraph is taken from the *The Times Engineering Supplement*, March 16th, 1910:—An electrically-operated time recorder used for registering the times of the flying-machines at the Johannisthal Aviation Meeting (September 26th—October 3rd, 1909) consists of a paper tape about 30 cm. in width moved along slowly between two drums by clockwork. A clock with contact device actuates, each second and each minute, two electro-magnets located beneath

The Late Lieut.-Colonel H. W. L. Moedebeck
Born 1857, died 1910

the edges of the paper tape, thus producing at both edges a continuous series of dots, the intervals between which correspond to a second and a minute respectively. The tape moves past styles which draw a number of parallel lines. Other magnets working styles arranged in one line across the tape are connected both to the umpire's cabin and to the points of observation distributed along the aerodrome, where timekeepers are stationed. Whenever a flying-machine starts and passes the goal line, the umpire presses a button bearing its number, and thus actuates simultaneously four electro-magnets of the recording device, so as to produce markings

on the corresponding section of the paper. The other points of observation also have push-buttons bearing the numbers of the machines. As soon as the timekeeper notes one of these in his field, he presses the corresponding button, and a dot is made on the paper in the particular section. The distance of this dot from the four dots made by the umpire at the start gives the time taken by the machine to arrive at the first point of observation. The same process is repeated at the next station, and so on, until four dots are again made simultaneously on the goal being passed.

The difference between the last four and the first four dots gives the time taken for completing one round. An exact record of the time and speed of the flight is obtained by drawing a zig-zag line connecting all the dots. When several machines are in the air simultaneously, a glance at the paper tape suffices to show which flew longest and fastest.

The Aero Show at Olympia—From every point of view, the second International Aero Exhibition, held at Olympia from March 11 to 19, proved an unqualified success. If originality in design was scarcely pre-eminent, more especially among the British-built aeroplanes, this only affords an instance of the progress and soundness of the industry, which is now devoting its entire attention to reliable workmanship and solidity of construction, rather than to startling departures from the lines of accepted practice. The number of machines exhibited was thirty-one, divided into 21 monoplanes, 9 biplanes, and 1 triplane. Of these, 14 monoplanes, 6 biplanes, and the triplane were of British construction. The prices of these machines ranged from £300 to £1100; the power from 20 to 60 H.P.; the weight from 300 to 900 lbs. net. As a rule, curiously enough, the price works out at about £1 per lb. weight. A tribute must be paid to the neatness and finish of the workmanship of the British-built machines; in these respects they have no superiors. If the same cannot be said, in some cases, of the design, this, no doubt, is a fault which will be remedied in due course, when the invaluable aid of practical experiment has been invoked. One fact was, however, glaringly apparent: many British constructors, in designing their machines, obviously disdain the help of science, preferring rather to proceed by rule-of-thumb methods—a practice that is bound, sooner or later, to end in disaster. There is no need, on our part, to particularise in this respect: time and practice alone will soon demonstrate such obvious errors. On the other hand, a tribute is due not only to the excellent workmanship of the majority of the British machines but to the originality of design in many instances. The George and Jobling biplane, the solidly-built Short biplane, the Spencer monoplane (driven by twin tractor-screws), the Petre monoplane (driven by a propeller situated aft of the tail), and the beautifully-designed Humber monoplane (Le Blon type) may be singled out in this respect. A number of British and foreign engines were displayed, and a host of parts and accessories. Mr. Willows' small dirigible and two spherical balloons were the only representatives of the branch of aerostation, which seems to suffer from momentary neglect in this country.

The Aëronautical Society's reception room proved a great convenience to the members, while the Society's exhibit of historical documents provided an undoubted attraction to the public.

Heliopolis Aviation Week—We have received the following communication from Mr. F. P. Walsh, C.E., who kindly undertook to represent the Society at the Heliopolis Meeting, February 6 to 13:—

"To a spectator who had had no previous experience of flying, the Cairo Meeting must have been enlightening, not as to the extent the art has progressed, but the reverse.

"Of all the competitors one only seemed to be able to fly when, and for as long, as he wished, namely, Rougier, on his Voisin machine; of course, no one ever ventured out when there was anything more than the very mildest breezes. Grade on his small and neat machine, seemed, next to Rougier, the competitor with the most command. Latham was a conspicuous failure. Great things had been expected of him, but he did practically nothing, as he never succeeded in getting his engine to pull properly.

"Sands seemed to fly when he wanted, but his performances appeared to lack confidence.

"The success of the meeting were undoubtedly the Voisin machines (Rougier and Metrot), next the Blériot (Le Blon and Balsan). Of the others, with the exception of Grade, not one particularly distinguished itself.

"There was no Wright machine entered."

Balloon Accidents.—On the morning of Sunday, April 3, the balloon *Pommern*, of the Pomeranian Aero Club, ascended from Stettin, in Germany, with the four following passengers: Dr. Delbrück (member of the Reichstag), Herren Semmelhack, Benduhn, and Heim. No sooner had the balloon left the ground than it came into contact with a line of telegraph wires and, immediately after, with the chimney stack of a factory. These collisions not only caused serious injury to the pilot, Dr. Delbrück, but entangled the valve-line and tore apart the netting. The valve having been put out of action no landing could be effected, but, a few hours later, when the balloon had passed over Sassnitz, on the Island of Rügen, and was being driven out over the sea, the ripping-cord was pulled and the balloon descended in the

The Society's Stand and Reception Room at the Aero Show, Olympia

Aero Show, Olympia: General View

water. Owing to the rough sea it was found impossible to reach the four passengers in time; Herr Semmelhack alone was eventually rescued. The comparative inexperience of those in charge of the balloon, together with the stormy weather, would seem to be responsible for a fatal accident, the more distressing in that it robbed aerial navigation of a distinguished patron.

On the same day a stupid accident caused the death of Professor Abegg, of Breslau University, who, with two companions, had ascended from Breslau. A landing was safely effected near the Pomeranian coast, but as soon as the two passengers had left the car, a violent gust carried off the balloon, which was thrown against a tree, and Professor Abegg, the pilot, who remained in the car, was killed.

We have to chronicle a further balloon accident which occurred in Germany only two weeks later On the evening of April 16 the balloon *Delitzsch*, belonging to the Bitterfeld Aero Club ascended from Bitterfeld with four passengers: Herren Karl Luft (pilot), Lüschenring, Hoecker, and Graupner. Early the next morning the wreckage of the balloon was found near Reichensachsen, some 80 miles west of the starting place. The four occupants had been killed, the balloon having apparently fallen from a great height during the violent thunderstorm that raged during the night.

The Death of M. Le Blon—By a lamentable accident M. Le Blon, one of the most brilliant of the recent school of aeroplane pilots, met his death at San Sebastian on April 2. Engaged to compete in the meeting that took place at this town from March 29 to April 1, M. Le Blon had carried out several flights on previous days, and on one occasion had descended in the sea owing to the failure of the motor. This incident, however, was not attended with serious results. But on the afternoon of Saturday, April 2, Le Blon, though his contract had expired, determined, with his usual courtesy, not to send away disappointed the crowd of spectators who had assembled. Heading out to sea in his flight, he rose to a height of 150 feet, when in the act of turning towards the shore, just opposite the palace of Miramar, his *Blériot*

Humber Monoplane (Le Blon Type)

This monoplane was designed by the late Hubert Le Blon for the Humber Company of Coventry. The most striking feature is the tubular fuselage built up of hollow cylindrical sections of pine wood telescopically assembled, the whole being bound with tape and varnished. Dimensions: Main surfaces, span 26½ ft.; area 194 sq. ft.; length of fuselage 24½ ft. Motor: 35 H.P. Humber. Weight (without pilot and petrol) 495 lb.

monoplane wavered and fell rapidly to the sea. Unable to regain control over his machine, M. Le Blon was submerged, entangled, it seems, in the aeroplane, and when help arrived had been drowned. Precise explanation of the accident, in the absence of direct testimony, is lacking. Deeply deplorable is the death of Hubert Le Blon, who, born in Paris in 1872, had, after a brilliant motor-racing career, placed himself in a comparatively short time in the very front rank of aeroplane pilots. In the few months that have elapsed since the Rheims meeting, at the conclusion of which he made his first flight, Le Blon flew his Blériot monoplane, in association with the late Léon Delagrange, at Spa, Doncaster, and Heliopolis, where he proved his brilliance as a pilot.

NEW BOOKS AND PUBLICATIONS

* Aeronautical Classics: No. 1 Aerial Navigation. Sir George Cayley, Bart. No. 2 Aerial Locomotion. F. H. Wenham. London: Aëronautical Society of Great Britain. 1910. 1s. n. each

* Aeronautics (Cantor Lectures, 1909). C. C. Turner. London: Trounce, 1910. pp. 48. 1s. n.

An der Wiege de Luftschiffahrt Francesco Lana. B. Wilhelm. Hamm: Von Breer, 1909. pp. 26. 50 Pfennig

Annuaire des Sciences Aéronautiques, 1910. A. Bracke. Paris: Vivien, 1910. pp. 96. 2.50 francs

Aviation in France in 1908. P. R. Jourdain. Washington: Smithsonian Institution. 1909. pp. 145-159

* Bulletin de l'Institut Aérodynamique de Koutchino. Fascicule 3. St. Petersburg, 1910.

Ceux qui ont volé et leur Appareils. Paris: L'Aéro, 1909, pp. 185. 2 francs

Die Aerologische Expedition nach Ostafrika im Jahre, 1908. A. Berson. Braunschweig: Vieweg, 1910. pp. XXXIII. + 119. 10 marks

Die Anfänge der Luftfahrt. B. Wilhelm. Hamm: Von Breer, 1909. pp. 204

* Die Berliner Flugwoche und einige ihrer Lehren. J. Hofmann. Berlin: Simion, 1909. pp. 10

Die Winde in Deutschland. R Assmann. Braunschweig: Vieweg, 1910. pp. 61. 5 marks

* Etudes Expérimentales sur les Zooptères. Vols. 1 & 2. P. Amans. Paris: Vivien, 1909. pp. 89. 1910. pp. 31

* First Lessons in Aeronautics. M. K. Kasmar. Chicago: Amer. Aer. Soc., 1909. pp. 159. $2.00

* How to Build an Aeroplane. R. Petit. London: Williams and Norgate, 1910. pp. 118. 2s. 6d. n.

L'Aviation. Commandant P. Renard. Paris: Dunod and Pinat, 1909. pp. 184. 5 francs

* L'Aviation Militaire. C. Ader. Paris: Berger-Levrault. 1909. pp. 161. 2.50 francs.

L'Aviation Triomphante. D'Estournelles de Constant. Paris: Librairie Aéron., 1910. pp. 331. 3.50 francs

Le Mécanicien Aviateur. Nos. 1-6. A. de la Hault & A. Bracke. Obourg: Conquête de l'Air, 1909-1910

Les Aéroplanes: Considérations Théoriques. P. Raybaud. Paris: Vivien, 1910. pp. 24. 1 franc

Les Cerfs-Volants. J. Lecornu. Paris: Vuibert, 1910. 2me éd. pp. 295. 3.50 francs

Le Vol ramé et les Formes de l'Aile. Commandant L. Thouveny. Paris: Berger-Levrault. pp. 35. 1.25 francs

* L'Organisation en France de la Locomotion Aérienne. P. Painlevé. Paris: Librairie Aéron, 1910. pp. 19

* Ortsbestimmung im Ballon. A. Marcuse. Berlin: Reimer, 1909. pp. 67

* Petite Encyclopédie Aéronautique. L. Venton-Duclaux. Paris: Vivien, 1910. pp. 143. 1 75 francs

* Sur la Résistance de l'Air. G. Eiffel. Paris: Librairie Aéron., 1910. pp. 98. 6 francs

* The Aeroplane Portfolio. D. Ross Kennedy. London: P. Marshall and Co.. 1910. 1s. n.

The Present Status of Military Aeronautics. Dr. G. O. Squier. Washington: Smithsonian Institution. 1909. pp. 117-144

Theorie und Berechnung der Luft Schraubfn. C. Eberhardt. Berlin: Krayn, 1910. pp. 122. 6 marks

Uber den Einfluss der Luftdichte auf den dynamischen Flug. A. Jarolimek. Wien: Feige, 1910. pp. 3

* Vehicles of the Air. Victor Lougheed. London: Fisher Unwin, 1910. pp. 479. 12s. 6d. n.

* Notice of this book will be found in the present number.

REVIEWS

Recherches Experimentales sur la Résistance de L'Air. Exécutées à la Tour Eiffel. By G. Eiffel. (Paris: Librairie Aéronautique, 1910. pp. 98. 6 francs.)

This publication gives an account of experiments made by M. Eiffel on the well-known tower designed by him and bearing his name. The plan adopted was to stretch a cable from the top of the tower to the ground below, and using this cable as a guide to allow bodies to fall from the top to the bottom and then to ascertain, by self-recording apparatus which shared the fall, the velocity, and the resistance recorded. For this purpose a drum on which the records were made was turned by the falling body in such a way that a definite angle was turned through by the drum for a definite length of fall along the cable. The time was recorded on the drum by a pen attached to the end of a tuning fork making one hundred oscillations per second. The plate or body on which observations were to be made was held by a spring and the deflexion of the spring also recorded on the same drum. Thus the record relating to each experiment gave the length of fall from the start, the time after which this fall was accomplished, and the force acting on the plate at the same instant. These quantities suffice theoretically to give the air resistance in terms of the velocity for each point of the fall, and in practice they do give this relation, with an accuracy that seems amply sufficient, for the latter part of the fall. Full details of the way in which the various difficulties were overcome are given, and the methods seem to me in all respects save one extremely good. The observations were made on velocities that were not uniform but were increasing somewhat, and it is not unlikely that the resistance in such a case is not quite the same as for a uniform velocity. The results are extremely interesting and are in good agreement with previous results.

M. Eiffel finds for the velocities dealt with, which range from about 40 to 80 miles per hour, that the resistance varies very closely with the square of the velocity, although the rule is not quite exact; the value of K in the formula $R = Kv^2$ altering by about 3 per cent. It seems possible this variation may be due to the want of uniformity in the velocity. The author also finds a greater proportionate pressure upon large surfaces than upon small ones. This is a point upon which considerable differences of opinion exist, different experimenters having come to different conclusions.

The actual value given for the resistance upon a square plate of $\frac{1}{2}$ square metre surface in metric units is ·073. This is equivalent to $R = ·0029\ v^2$ where R is the resistance per square foot, and v the velocity in miles per hour. The value accepted by the Meteorological Office is $R = ·003\ v^2$. Since this value is based upon experiments made upon a plate of 1 square foot (which equals $\frac{1}{11}$th of a square metre) the agreement given by two totally different methods of experiment is very close.

A very interesting curve is also given showing the pressure upon planes inclined at various angles; the curious peak that is known to occur at an angle about 35° is apparent on this curve.

There are two notes at the end; one gives a table of values found by previous workers, and the other some useful formulæ concerning the rate of fall of a body under the combined influence of gravity and air resistance.

W. H. DINES.

L'Aviation Militaire. By C. Ader. (Paris: Berger-Levrault, 1909. pp. 161. 2.50 francs.)

Every student of aeronautics is familiar with the history of M. Ader's early attempts at aviation which, as long ago as 1891, so impressed the French War Minister of the time that he decided to continue the experiments at the public expense with a view of introducing flying machines into the army. Unfortunately, however, the time was not yet ripe; the necessary faith in the ultimate achievement of dynamic flight was not forthcoming, and, the second flight of the "Avion" ending in disaster, the Minister of War abandoned the experiments. The machine has been preserved in the Conservatoire des Arts et Métiers where it may still be seen, and, but for the timely appearance of the book under review, would have been the only record of a close study of the subject extending over many years.*

Ader recognised from the first the enormous potential value of flying machines from a military standpoint, and set himself the formidable task of designing a complete series of "avions" (the word he adopts in preference to aeroplane) that would meet all the requirements of military strategy and tactics in so far as he judged that they could be met. Further than this he proposed to found schools of military aviation and "avionnerie," and arsenals for the construction of avions and their armament.

* The author also published, in 1893, an account of his early experiments and projects under the title "La première étape de l'aviation militaire en France," of which the introduction to the present book contains a *résumé*.

The elaboration of a scheme on these lines appears to have been begun shortly after the "Eole," his first avion, had made a flight of 100 metres in 1891. During the years that followed a number of notes were made on the various aspects of the problem, and some of these have been collected and published in the present volume.

The first Note, written in 1898, describes the various types of avions proposed. These are divided into three categories: scouts, torpedo-vessels, and "avions of the line." Some of the characteristics considered by the author to be essential in all types are well worthy of the attention of present-day designers. For instance, wing frames jointed in such a way that, not only can the over-all dimensions be much reduced for housing and transport purposes, but, perhaps more important still, so that the wing-area can be reduced or increased at will during flight, thus enabling high speeds to be attained when necessary to fly against strong winds, and slower speeds for starting and landing in light winds. In the case of scouts—vessels in which speed would be the dominating factor—it is proposed that the wing-area be variable in the ratio 5 to 1 at least, the intention probably being to obtain a somewhat smaller ratio, say 4 to 1, between the maximum and minimum speeds; though, of course, this can only be inferred from other evidence, as no details are given showing whether reduction of wing-area was to be accompanied by any change in the angle of incidence of the wings to the flight-path. It should be noted, however, that the author estimates that his "avions d'élite" of the scout class would have easily attained a speed of 200 kilometres an hour. Nor would the present-day student of aviation consider this figure unattainable, in fact the late Captain Ferber confidently foreshadowed the attainment of as much as 300 kilometres an hour, remarking that this speed would be no more difficult for an aeroplane than 100 has proved to be for an automobile.

Everything in the scout class was to be sacrificed to speed and radius of action, and therefore the armament was to be confined to hand-grenades, the type preferred consisting of two grenades connected together by a cord, the tension of which would cause the grenades to explode when the velocity of one of them was suddenly arrested by striking some portion of a hostile avion. It is curious that in no case are machine guns, or even ordinary rifles, proposed as part of the armament of any type of avion. The torpedo vessels, which would be the largest and most heavily armed class, were to carry torpedoes of various sizes weighing from 1 to 100 kilogrammes each, and arranged to be dropped vertically by an ingenious gear designed to release their weight gradually and so avoid sudden disturbance of the equilibrium of the avion. In addition to this they were to carry some kind of star shell to illuminate the ground when making a night attack or a reconnaissance, but the use of a missile of any kind propelled by any other force than gravity never seems to have been contemplated by the author.

The function of "avions of the line" is somewhat obscure, but they were intended to combine, as far as possible, the speed of the scout with the destructive power of the torpedo-vessel. They were also to carry a peculiar weapon taking the form of large numbers of sharp-pointed steel darts from 10 to 20 centimetres long—knitting needles, in fact—fitted with little helically-shaped tails designed to ensure their falling point first by causing them to revolve on their axes.

Space will not permit mention of the many details discussed in this Note. It must suffice to say that they comprise many points of design from the propellers to the chassis, and include such points as methods of signalling and of sighting on a target for the accurate dropping of torpedoes.

The second Note deals with aerial harbours, that is to say, places prepared for launching and landing of avions, together with provision of shelters and arrangements for protecting them from attack. These harbours are divided into three kinds, viz.:—Permanent fortified harbours, field harbours, and portable harbours. In the case of the first-named the ground would be carefully levelled, the surface macadamised or asphalted or wood-paved, and arrangements made for lighting and signalling at night, and also for indicating the direction of the ground wind.

Field harbours would be of a similar but more temporary nature, the ground not being so elaborately prepared, and the shelters for avions and magazines less strongly protected.

Portable harbours would consist of wood platforms for use as starting and landing places, shelters for the avions, and tents for officers and men, all carried in sections with the transport of the army corps.

The third Note is a short one on avions for use in naval warfare on the high seas. Vessels of special design to carry them are proposed, and details are given of the method of starting from and landing on the decks of these vessels. These avions would carry torpedoes of similar design to those already described but with the addition of an arrangement enabling them to be exploded at a predetermined depth below the surface of the water.

The fourth Note discusses the design of artillery for attacking avions from the ground. The author has little faith in the hitting power of these "vertical fire" guns, but considers that they would be of use in keeping the enemy's aerial torpedo craft at a distance.

The fifth Note discusses at some length what the author calls the "aerial routes"—that is, the lines along which, owing principally to the configuration of the ground, permanent rising currents of air are established when the wind blows from certain quarters. It may be thought that the author overestimates the economy of power that flying machines can obtain by utilising

these rising currents, but there is no doubt that we have much still to learn in this direction. The second part of this Note is enlivened by some amusing experiences of the author in his travels in search of soaring birds, but his observations are very instructive, especially those on large vultures in Algeria.

The sixth Note concerns the proposed schools of aviation and "avionnerie," the former being for the training of officers and others as military aviators, and the latter for the engineers who would design and construct the avions, their armament and accessories.

The seventh and last Note deals with strategical questions and outlines a scheme of distribution of an army of avions for the defence of France.

The forecast of the waking-up of England, and her feverish activity to obtain the mastery of the air when at last she realises that her "splendid isolation" has gone, will not perhaps convince English readers, principally because it is based on what most of us would consider a highly exaggerated view of the destructive potentiality of explosives dropped from flying machines. Moreover, the whole subject is treated far too much as if war on land and sea were likely to cease except for the purpose of attack on and defence against "avions." It is curious, too, to note that the dirigible balloon finds no place in the author's calculations.

A. D. C.

Proceedings of the Aerodynamic Institute of Koutchino. 3rd Volume (St. Petersburg: 1910.)

The latest report, giving an account of the last year's work, is at once interesting and disappointing.

The first article relates to investigations made to determine the momentary pressure exerted by a current of water on a fixed plate suddenly exposed to its action.

A suitable float is allowed to drift down with the stream, a vertical plate being fixed to the underside of the float, in a plane at right angles to the direction of flow.

The float and plate are suddenly brought to rest by coming up against suitable obstacles placed in the stream, consisting of a system of piles and cross bars.

The plate is fixed and suspended under the float in such a manner that the pressure exerted on it when it is thus suddenly brought to rest is registered on a revolving drum carried on top of the float.

By these means curves are obtained, showing that the pressure of the water on the plates leaps from zero to a maximum when the plate is brought to rest, and then falls speedily to its normal value.

This experiment is unfortunately rather typical of the great majority of the researches described in these volumes. It is difficult to see what ultimate aim the originators can have had in their minds.

The next article is a note on the construction of wind tunnels in which aerodynamical experiments are to be carried out. This should prove useful to those who intend establishing an aerodynamical laboratory. Figures are given showing the influence of the sides on the results.

The next article gives the results of investigations of the pressures exerted on an oblong rectangular plate which is revolving about its longitudinal axis in a current of air. Once such a plate is set spinning, it develops autorotation. This phenomenon is compared with the gyrations of a similar plate if it is allowed to fall freely from a considerable height in still air; it does not glide down, but develops a regular periodic motion accompanied by rotation. Plane plates, and combinations of plates, have been placed in air currents, and the pressures noted and analysed. There appear to be a number of different explanations of these phenomena, and the present set of experiments have led to yet a fresh one.

The next article is the origin of the proposals that have appeared in a number of aeronautical papers, describing a novel method of exploring the air currents of the atmosphere.

The proposal is that special balloons should be sent up, carrying a large number of small flags; the balloon will automatically drop a flag at fixed intervals of time, throughout a period of days. For the guidance of dirigibles such a procedure should be most valuable. The track described by any given particle of wind which forms part of, say, a cyclonic system, has never yet been thoroughly elucidated. As soon as dirigibles reach the stage in which they seriously attempt long ocean voyages a knowledge of this and similar matters will be as vitally necessary as was the elementary knowledge of the laws of storms to the masters of sailing ships. To be without such knowledge has almost always spelt disaster, and the history of commerce is full of examples of ships under identical conditions making the storm in the one case their friend, in the other their destroyer, according as they knew or ignored these all-important laws. Just as Government money has been spent on tracking the ocean currents, so it will have to be spent in these newer fields. The method suggested by the Koutchino Institute seems well adapted for the purpose, pending the time when private and public enterprise will send out airships, to keep the air for weeks or months, drifting with nothing to do but plot and analyse their tracks.

The next article describes an apparatus for measuring the change of the resistance of a submerged body with change of pressure of the fluid. No results are given.

Finally some 54 photographs are given showing the flow of air past various objects immersed in the current.

These photographs are so beautifully executed, and show so clearly all the vortices and disturbances, that it is to be regretted that scarcely any of these 54 experiments deal with a shape or arrangement of any use in practical aeronautics. There are two photographs of a single plane and two of a single aerocurve placed at practical angles in the stream. Apart from these, the photographs show some object or combination of objects which are of no direct practical value. When one considers the superposed planes, the "dipping front edges," the complete models of aeroplanes and dirigibles, the stays, wires in, and not in, vibration, and the hundred other objects and combinations concerning which such invaluable information could have been obtained, one can only regret that science and practice should not have better combined, and express the hope that all these researches may prove to be merely the solid foundations for future and more valuable work. N. F. U.

Vehicles of the Air: A Popular Exposition of Modern Aeronautics, with Working Drawings. By Victor Lougheed, member of the Aëronautic Society of the United States. (T. Fisher Unwin, London; 12s. 6d. net.)

With this book before us we have no hesitation in acknowledging that we can credit the United States with the best popular work on aeronautics yet published. The price may not be held to be popular in the general sense of the word, but the material in this book, the admirable way in which it is arranged, and the lavish and beautifully-executed illustrations make it essentially a very cheap volume. So much for a superficial estimate. Perusal of the contents does not in the least disappoint the high expectations provoked by the general appearance of the book. The exposition of the principles of aeronautics in every division of the science is absolutely sound so far as they are known. Where they are not known Mr. Lougheed is careful to say so; and since his style of writing is lucid and agreeable and is not marred—from the British point of view—by gross conversational Americanisms which too often find their way into American books, this work ought to appeal to the English-speaking people everywhere. Naturally, there are differences of expression and one or two curious technical terms, but none of these are likely to irritate even a sensitive reader. One fault may be mentioned, the exhaustive list of flight records at the end of the book is printed in so small a type that it is almost unreadable. And there is one curious mistake, in the detailed drawings of the Antoinette machine on page 101 the letterpress concerns the Curtiss biplane, while on page 397 drawings of the Curtiss machine are given with the description of the Antoinette.

The headings of some of the divisions will give an idea of the scope of this work and will show how conveniently it is arranged. The introduction deals in a logical way with the human and mechanical limitations of aerial navigation. The atmosphere is then discussed, and this chapter is followed by one on lighter-than-air machines. Mr. Lougheed's consideration of the dirigible balloon occupies only 40 pages, in which short space, however, he contrives to be very comprehensive and informing. Chapter III. concerns heavier-than-air machines, and this is followed by one on aeroplane details, a division that is sub-divided into parts dealing with forms of surfaces, arrangement of surfaces, methods of balancing, steering, and controlling means, so that we are given a very complete survey of theory and of the mechanical application of theory. Other chapters deal with propulsion, "power plants," transmission elements, bearings, starting and alighting, materials, accessories, etc. The illustrations of the various parts of machines are remarkably well done, and the illustrated descriptions of the principal aeroplanes with working drawings are clear and accurate. The book has the merit of being free from bias, national or other, and the prominence given to the important work of Montgomery is an excellent feature. Perhaps it was not quite necessary to discuss, in such a work, lighting systems, fog-horns, and whistles; but, after all, that is only a matter of opinion. In one or two minor matters of conjecture readers may not agree with Mr. Lougheed. For instance, he contends that the cost of flying and the depreciation of machines will be little. This happy state of affairs seems at present to be rather remote. He says it is well established that hydrogen balloons have been set on fire by electrical discharges from the atmosphere. Surely there is only one instance, and that a doubtful one. With one thing he says we shall all agree, "Scepticism is ignorance. Aerial navigation to-day is no more a joke than was the railway eighty years ago or the automobile ten years ago."

And like these its development and application will be "of the profoundest import to every phase of civilisation and every activity of the race." C. C. T.

Etudes Experimentales sur les Zoopteres. By P. Amans.—Part. I. (Paris: F. L. Vivien, 1909.)

This work by Dr. Amans is an interesting one, as it deals with a side of aeronautics that has hitherto been very little studied in detail, viz., the effect of air currents on the wings of birds

and insects. The author is an enthusiastic believer in the bird or insect type of flier, and the pamphlet contains fairly full details regarding the results obtained by him in his experiments.

In Chapter I. the "Zooptère," or model wing form, is described, and the method of constructing it. Generally speaking, the models are formed more or less like the wings of insects, viz., with "nervures" or supporting ribs, but the type of rib varies with the class of experimental wing.

In Chapter II. the undulating curves present in all wing forms are discussed, and the application of these curves considered.

Chapter III. gives an account of "Rigid Wings," explains the principle of their construction, and gives details regarding certain insects.

Chapter IV. is on the flexibility and curvature of wings, a difficult subject, which has hitherto received very little attention. The form, torsion, etc., of different kinds of feathers and wings are discussed, and the action of the air upon them explained in detail.

In Chapter V. the results of some experiments made with wing type propellers are given. The pressures and velocities of the air stream formed by the propellers were measured at suitable intervals, and the author shows that with this type the velocity of the stream is constant at quite a short distance in rear of the blades, not only along the axis of the screw, but also at quite appreciable distances on each side of it.

Chapter VI. gives an account of dynamometric experiments made on the type of propellers above described, and shows how advantageous they are.

The pamphlet is well worth studying, and it is to be hoped that members of the Aëronautical Society will take the subject up and further develop Dr. Amans' ideas.

Etudes Experimentales sur les Zoopteres. By P. Amans.—Part. II. (Paris: F. L. Vivien, 1910.)

This is a continuation of the former pamphlet, and contains a description of the apparatus used by Dr. Amans, with some results of the experiments carried out by him.

He points out the objections to whirling tables, funnels, etc., as used by other experimenters, and shows how the wing type of propeller described in his earlier pamphlet can be used for testing bodies and surfaces. The results of his experiments are interesting, and it is to be hoped that a more detailed account of them will be published shortly. J. D. F.

L'Organisation en France de la Locomotion Aerienne. By P. Painlevé (Paris: Librairie Aeronautique, 1910; pp. 19.)

This little patriotic pamphlet is a first-class specimen of the way in which propaganda work is carried out in France. In short, pithy paragraphs M. Painlevé asks why Germany is ahead of France in aeronautics, lays a finger on the sources of her strength, and finally marshals a comprehensive programme for the French Government to carry out.

The three reasons given for Germany's superiority are (1) the immense sums she is willing to spend (2) the splendid organisation of the industry, which is always being improved and perfected down to the smallest detail, and (3) the moral factor—the disciplined enthusiasm, the heartiness, perseverance and initiative, which animates all those employed.

Germany has many hundreds of men trained in the handling of dirigibles; she can manœuvre her airships expeditiously and safely, for she has learnt in the school of experience, and paid the highest prices for the best tuition. The chemical industry and factories producing hydrogen as a by-product are subsidized; and the train service for the rapid transportation of gas-tubes from point to point has been completely organised.

M. Painlevé's programme for France is a big one, but it is only adequate. He asks for 15 or 20 million francs. Hangars, large and small, must be erected, and 10 airships of various types built at once. The whole of the aeronautical establishment must be increased and reorganised, in the civil as well as in the service departments. The Government should encourage the industry by a bounty system and subsidies, and arrange that private air-craft be at the disposal of the authorities in war time.

With regard to aeroplanes, the purchase of a large number of aeroplanes of all types for the voluntary training of men in both services is recommended, and also the institution of a national aeronautical laboratory with a flying ground attached.

"The country," cries M. Painlevé, "that neglects the dirigible is imprudent; the country that ignores the aeroplane shows an entire lack of foresight."

As a means of informing and stirring up the public, we can imagine nothing better.

T. O'B. H.

How to Build an Aeroplane. By R. Petit. Translated from the French, by T. O'B. Hubbard and J. H. Ledeboer. (London: Williams and Norgate, 1910; pp. 118. 2s. 6d. n.)

As the first treatise in English on aeroplane construction this book, attractive in appearance and moderate in price, should have a large sale. It has faults and shortcomings, and its title is

somewhat misleading—it should be "How Aeroplanes are Built"—but taken as a whole, it will give the non-technical reader a very fair idea of how flying machines are made, besides providing, for those more advanced, wrinkles and theories that will be of practical use to them.

The first chapter deals with the general principles of aeroplane design, going into elementary mathematics, which, while conveying little fresh information to the expert, will undoubtedly leave the lay-mind convinced that it has learnt a great deal. The other seven chapters, which are designed entirely for the novice, give a good deal of useful information about materials, the various propellers, motors, controls, and chassis in use, and the manufacture of the planes and fuselage, and also describe their assembling.

The format and printing could not be bettered, the translation is quite admirable, and there are nearly a hundred good illustrations.

Perhaps M. Petit has made a mistake in trying to appeal both to the expert and to the novice in a book of this description; there is always a danger of falling between two stools in such a case. This slight error of judgment, however, will scarcely impair the book's success.

R. C.

Aeronautical Classics. No. 1. AERIAL NAVIGATION. By Sir George Cayley, Bart. No. 2. AERIAL LOCOMOTION. By F. H. Wenham. (London: Aëronautical Society of Great Britain, 1910. 1s. n. each.)

Realising how much time and money is wasted in aeronautics nowadays by the neglect of would-be inventors to study the work of the pioneers, the Aëronautical Society is publishing a series of reprints of which the first two volumes are now published. Their price, printing, and appearance leave nothing to be desired. Both contain excellent portraits of the authors, useful biographical notices, and all the original illustrations in facsimile. Members should not hesitate to subscribe to this first-rate series.

We understand that any profit—there cannot be much at so popular a price—is to be devoted to the Society's Library.

Ortsbestimmung im Ballon.—By A. Marcuse. (Berlin: Reimer, 1909; pp. 67.)

This most interesting little book deals with the fixing of the position of a balloon or airship by means of astronomical observations. While passing over the theory of this method of navigation, the author presents a large number of original tables and diagrams by which the calculations involved may be reduced to a minimum—to the work of a couple of minutes in fact.

The author states that by such observations an aeronaut can rely on fixing the position within 10 kilometres. But this is the estimate of an enthusiast who has devoted years to practice in the work. Others of less experience generally agree in considering that they have done well if they fix the position within 30 miles. This book should be of use to seamen in shortening their reckonings.

Aeronautics. (Cantor Lectures, 1909.) By C. C. Turner. (London: Trounce, 1910. pp. 48. 1s. n.)

These Lectures, delivered during November and December last year, fully substantiate in print the good impression that they made orally. Dirigibles and aeroplanes are examined in a popular but thoroughly sound manner, and the tyro desirous of mastering the first principles of aeronautics will find in this brochure simple and satisfying answers to his questions of "how does it work?" and "of what good is it?"

First Lessons in Aeronautics. By M. K. Kasmar. (Chicago, 1909: The American Aeronautical Society; pp. 159—$2.00.)

The three essential qualities that go to make a sound elementary text-book are sound scientific knowledge, clearness of expression, and simplicity of arrangement. It must be confessed that this work is conspicuous for none of these qualities. At any rate, the first of the three qualities is, in this case, destroyed by the absence of the other two; and the whole work may be described as a painstaking reiteration of the obvious.

The Aeroplane Portfolio. By D. Ross Kennedy. (London, 1910; P. Marshall and Co., 1s. net.)

Contains nine plates of scale drawings of the following aeroplane types: Wright, Farman, Voisin, Curtiss, Cody, Blériot, Antoinette, Santos-Dumont, R.E.P. These scale drawings are accompanied by a short descriptive text giving the principal features and dimensions of each machine together with the most noteworthy performances that stand to its credit. The usefulness of this excellently-conceived production would have been greatly increased had it included details of such important features as the methods of construction of framework, etc., and some figures relating to the curvature of surfaces.

Petite Encyclopedie Aeronautique. By L. Ventou-Duclaux. (Paris, 1910. F. Louis Vivien, 143 pp.; 1 franc 75.)

This excellent little volume consists of an extensive glossary of aeronautical terms and appliances. The terms have been selected with care so that there is little redundancy. The entries include, in their proper place, detailed descriptions, with diagrams, of the principal dirigibles, aeroplanes and aerial motors.

Die Berliner Flugwoche. By Jos. Hofmann (M. Aer. Soc.). (Berlin, 1909: L. Simion).

A brief review of the Berlin Aviation Week (September 26—October 3) and of its lessons, in which the author criticises the sharp corners of the course (one of which had an angle of 60° and was productive of several mishaps) and from which he concludes the necessity of cutting down the preliminary run of an aeroplane on the ground by reducing the normal angle of incidence of the foils.

CORRESPONDENCE

THE BALL AND ROD EXPERIMENT

To the Editors of the *Aëronautical Journal.*

SIRS,—Cases are unfortunately not rare in which a natural phenomenon of unimpeachable respectability, so to speak, is utilised as a means of deceiving the public. The subject of this communication was so used not long ago by an enterprising gentleman who claimed to be able to influence matter, without actually touching it, by mental action. One of his feats was to blow upon a captive celluloid ball and make it advance *towards* him, *against* the current of air impinging upon it. This phenomenon is perfectly genuine and can be demonstrated by anyone who "knows how." It is, however, sufficiently startling at first sight to the uninitiated to endow the demonstrator with an entirely fictitious reputation for magical powers; the more so that attempts to imitate the feat would probably fail ignominiously from lack of knowledge of the underlying principle. An explanation of this is proffered below.

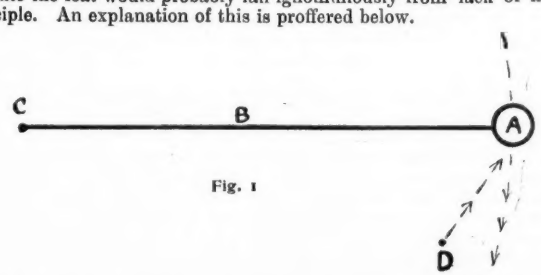


Fig. 1

The diagram (fig. 1) shows in plan the necessary apparatus. The ball A is supported by a rod B which is supported in its turn at axis C, round which it is free to swing in a horizontal plane. The demonstrator causes the ball to travel towards him by blowing on it from point D.

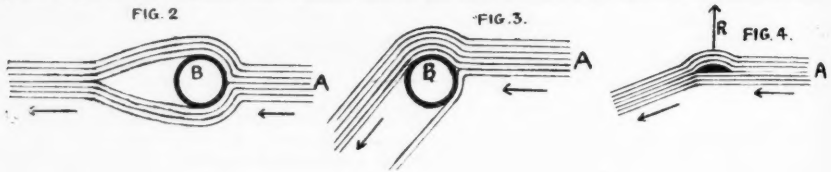
In order to appreciate why the ball so behaves it is necessary to consider what happens when a fluid stream issuing from an orifice encounters a sphere. It will simplify matters to consider its behaviour on meeting a cylinder (to which form the centre section of a sphere approximates) which we may imagine as extending an infinite distance either side of the plane of the paper; the whole fluid disturbance can then be indicated in plan. It should be noted that no attempt has been made, in the diagrams herewith, to represent the eddies and vortices which are at present in the actual fluid stream, both before and after meeting the obstacle, as they are not relevant to the explanation. The stream dealt with is not larger than the cylinder in cross section, as the power of diversion of the latter extends of course only to the region immediately adjacent to it.

If the fluid stream (shown at A, figs. 2-4) meets the cylinder (shown sectionally at B) "dead-on," fig. 2 represents what occurs. The original stream divides into two equal streams, which pass on either side of the obstacle, leaving its surface at, or just after, passing its maximum girth, and, skirting the dead-water region, join again on the far side. The resultant stream, being made up of two converging equal streams, takes the line bisecting the angle of convergence—that is, the same straight line as the original.

When the stream meets the cylinder "off-centre" a different result ensues. The original stream now divides into two unequal streams, the smaller of which, if the cylinder is struck sufficiently off-centre, is so much diverted from its path that it does not rejoin the rest of the stream. The larger stream shows the usual inclination to "hang on to" and follow the surface

of the obstacle, and is also diverted in the same direction as the smaller stream. The consequence is that the original stream is diverted from its path as shown, somewhat exaggerated, in fig. 3.

This effect is akin to that obtained from a round-backed blade. The portion of the cylinder or ball which does the diverting of the major portion of the stream is the off side, away from the axis C (fig. 1). If we cut off a section of this we have a round-backed blade, the action of which on the air is represented in fig. 4. As is well known, this shape of blade gives a decided reaction in the direction towards which the rounded face is pointing, even when the flat side is in direct alignment with the fluid stream impinging on the blade. As the flat side can in this case have no effect upon the fluid, the reaction is due to the diversion of the stream passing along the rounded face, to the contour of which it conforms, the resultant stream taking a line governed by the angle of convergence and relative size of the two streams into which the blade divide



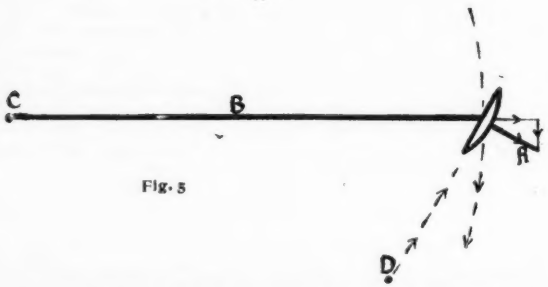


Fig. 5

the original stream. The reaction can be represented by a line R at right angles to the flat face (this is approximately correct), and we now have the clue to the behaviour of the ball.

If we were to blow on the ball "off-centre" from a position about axis C (fig. 1), the reaction R would obviously be at right angles to the pull of the supporting bar, and the ball would be given a strong impulse round the axis. On the other hand, blowing from a point at right angles to the supporting bar would lead to the reaction R being balanced by the pull of the bar. We have simply then to move so far towards axis C as will lead to a portion of reaction R being *un*-balanced by the supporting bar to ensure that the ball will come towards us. Fig. 5 (in which a round-backed blade is shown for clearness) shows this condition fulfilled.

BERTRAM G. COOPER

April 15, 1910

MAN FLIGHT

SIRS,—In answer to your letter, I beg to state that I delivered a lecture lately in the "Verein Deutscher Flütechnikie" about the further development of aviation.

My conclusions are based on the results of the measurements of the resistance of the air made by my deceased brother and myself, and published in "Der Vogelflug" (newly published by myself at R. Oldenbourg's Verlag, München). These measurements are exactly uniform with the results of correctly built modern flying machines, which I could prove by mathematical calculation. Regarding this statement, I claim that our further discoveries, published in the same book, are also reliable; especially I referred to the wonderful increase of the resistance of the air by beating wings, in opposition of the effect of the screw. I proved this by an experiment shown in the lecture hall. This stated a multiplication of the air pressure of seven times more than the general calculation otherwise shows. Based on this fact, I am able to prove that man can fly with his own force without a motor, as only one-third h.p. is required when a wind of at least six metres per second or more is blowing. I stated further that the question of equilibrium has to be solved before it is possible to make experiments, and that my special attention at present is occupied to construct a combination of special formed wings, which keep the equilibrium in the ever changing wind.

GUSTAV LILIENTHAL

"THE CONQUEST OF THE AIR"

SIRS,—Apart from personal privilege, a reply to a review is only justified when new facts are brought out for the benefit of readers. This is the author's excuse for replying to the review of "The Conquest of the Air," on pages 40 to 42 of the January *Journal*.

In the first place, the writer must express his satisfaction that the only portion of the book which is based on his own work, namely, the chapter dealing with the "Ocean of Air," is regarded so favourably. The greater portion of the book is, as the review states, a compilation from various sources, which, however, are believed to be reliable. Incidentally, it may be said that the nomenclature of the Zeppelin balloon, to which exception is taken, is correct, for it was not until the destruction of No. 4 that the new numbering was begun. Mention of the compartments in the rigid system was not omitted since on page 103, in speaking of the Zeppelin balloon, it is said: "It will be noticed that some of the ideas, which have been used to secure safety at sea, namely, the division into compartments and the duplication of the engines, were applied to this balloon." The most serious criticism, however, is the following: "Towards the end of this chapter (p. 122) the author springs upon us this axiom 'that the motive force increases as the *cube* of the speed for balloons of similar design and the weight per horse-power diminishes in the same ratio.' We should have liked to see how this was arrived at, especially, in view of the fact that in the example given the speed is doubled when the horse-power is squared." The importance of this subject seems to require a full explanation of the author's statement, taken from a recent book, "L'Aéronautique," by Comt. Paul Renard, than whom no one writes more accurately and clearly. He says: "The general law is that the motive force is proportional, not, like the traction, to the square, but to the cube of the proper velocity. To double the velocity of an aerial vehicle, other things being equal, the motor must be eight times as powerful, to triple it the power of the motor must be multiplied by 27, etc." The lightness of the motor is expressed by the quotient of the weight of the motor divided by the horse-power. Now, 30 years ago steam-motors weighed about 100 kg. per horse-power and consequently dirigible balloons were impossible. In 1884 Capt. Renard constructed an electric-motor of 44 kg. which was exceptionally light and gave a velocity of 6·5 metres per second. To double this speed and bring it up to 13 metres it was necessary to have the motor eight times lighter, or not much more than five kg. per horse-power. This is about what exists to-day, and consequently we see dirigibles attaining a speed of 12 to 15 metres per second. The same explanation is given by A. Berget in his "Route de l'Air," which has been translated in England under the same title as the author's work (and was reviewed in the October *Journal*). In the example criticised, if a velocity of 13·5 miles per hour requires 8·64 horse-power, with a maximum weight of 178 pounds, a speed of 27 miles per hour, or twice as much, will require 69 horse-power, or eight times more, and the weight per horse-power can only be 22 pounds, or about one-eighth of the original weight, for the same balloon.

The following sentence appears ambiguous to the reviewer: "In a balloon the carrying capacity is proportional to the volume, which increases as the cube of the surface and weight." A comparision of the advantage for transportation of the large balloon over the large flying machine was intended, and it was expected that the reader would remember this more ample statement in a previous chapter: "Since the lift of a balloon increases as the cube of the linear dimensions, but the surface, and consequently the resistance of the air, only as the square of these dimensions, it follows that the larger balloon with an equal speed will carry more weight, exclusive of its motors, than the smaller one."

As regards the omission of any discussion of lift and drift, aspect-ratio, stability, and the resistance of the air, etc., the author would say that his "Primer" is intended for the man in the street, and the publishers required its contents to be confined within 200 pages.

A. LAWRENCE ROTCH

Blue Hill Meteorological Observatory, U.S.A.
March 14, 1909.

THE DAGENHAM GROUND

SIRS,—As Chairman of the Experimental Ground Committee, I take strong exception to the remarks of Mr. Turner at the Annual General Meeting. He might have taken the trouble to ascertain the real facts before making such sweeping assertions.

He asks who chose the ground at Dagenham, and whether it was chosen by all the members of Council "in a body" or the Committee. The Council, of course, had nothing to do with it. It was chosen by the "properly constituted" Committee, all the members of which visited the ground—most of them two or three times—before it was definitely decided upon. The ground was regularly used by only four or five members (not "one or two"), and it is a very great pity that more did not avail themselves of it. Mr. Turner states that this

ground was not suited to its purpose. The Committee, who were in a better position to judge of this, were of opinion that, though it had its faults, it had all the makings of a really first-class ground—indeed, it is by nature quite the best to be found near London. But it would involve an expenditure of fully £300 to £400 to develop it into a good flying ground, which the Committee could not get. If a dozen members had come there and had given £50 each for the use of it, matters would have been different. Nevertheless, anyone who witnessed the trials with Mr. Moreing's Voisin machine there would agree that though the area was restricted, in experienced hands the machine could have been made to fly. A space of about 300 by 400 yards was available with perfectly open ground round it, but this was bounded by ditches. If these ditches could have been filled in a run could be made in any direction of fully 600 yards across the flat. As regards the upper part of the ground, which was covered with scrub, the scheme on hand, estimates for which had been gone into, was to clear a circular patch 300 yards across, to be rolled and cindered, and this would have made a perfect starting place. This was actually commenced by the construction of a good cindered track 400 yards long, and on this many trials were actually made with two different machines.

It must also be remembered that this ground was chosen not merely for practice with aeroplanes, but also for all other kinds of experiments in aeronautics, such as gliding, kite-flying, etc. There are other considerations, too, to be taken into account. Privacy, proximity to London, facilities for repair shops, lodging and feeding accommodation, etc., and it is not easy to find all these combined.

If only members would rally together and co-operate, instead of paying high prices for the use of grounds in various places, a good ground could be provided by the Society. But if they hold aloof and only grumble at what is provided by those who have taken a great deal of trouble to do their best, then no satisfactory results can be expected.

B. BADEN-POWELL

SIRS,—The question of the Experimental Ground is a very small part of the case for the appointment of the elected General Purposes Committee which was decided upon at the annual meeting; but since a great deal has been made of it, and the subject is likely to be referred to again in the *Journal*, I should like to make one or two remarks.

It seems idle, at this date, to defend the choice of the ground at Dagenham. In the annual report of the Society it was admitted to be a failure. Mr. Balston, during the discussion at the annual meeting, admitted that it was a failure. Mr. Reid used the words "you cannot blame anybody for being a little wrongin their estimate of the requirements of such a ground." Finally, we have the bill to pay.

The ground was chosen for flying experiments. It is admitted that it is not suited for that purpose. As to not blaming anybody, I think we are entitled to question the judgment of those who were responsible. After all, to an expert, or to experts, the choice of a ground for flying experiments should be quite a straightforward business. What is needed is perfectly well known, and should, I venture to say, have been known at the time this ground was chosen. The slightest element of doubt as to the suitability of the ground ought to have occasioned a pause in the negotiations for it. No doubt there were considerations which made it desirable that the ground should be selected quickly; and perhaps there is no need to criticise harshly those who were entrusted with this matter.

I only raised the subject because it illustrated the growth of the business of the Society and the need that we have of the help and advice of a Committee that can control business affairs, leaving the Council undistracted by them, and with more time, therefore, for the scientific objects of the Society.

CHARLES C. TURNER

Press Club, Wine Office Court,
Fleet Street, E.C., April 14, 1910.

THE FIRST AEROPLANE FLIGHTS

SIRS,—I have had my attention called to your issue of January. On page 40 you give the first aeroplane flights throughout the world. This is of particular interest to me, for my brother, Mr. A. V. Roe, was the very first to leave the ground on a free aeroplane in England. It was on June 28, 1908, that he made his best flight. This was about two feet off the ground for about 60 yards. Of course, it was not a long flight, but sufficiently long to be recorded as the first. He was then using a 24 h.p. Antoinette. This will place Great Britain fourth on the list instead of ninth.

In June, 1909, as you will, of course, know, he made the first flight ever made on an all-British aeroplane.

I am sorry I did not call your attention to this sooner, as it is only fair to my brother that he should have the credit of it.

H. V. ROE.

ANALYSIS OF FOREIGN PUBLICATIONS

ABBREVIATIONS

L'Aér., L'Aéronaute (Paris)
L'Aérop., L'Aérophile (Paris)
D.Z.L., Deutsche Zeitschrift für Luftschiffahrt (Berlin)
Flug, Flug und Motor Technik (Vienna)
Rev. Aér., Revue Aérienne (Paris)
Rev. de l'Av., Revue de l'Aviation (Paris)
Tech. Aér., La Technique Aérienne (Paris)
Aeron., Aeronautics (New York)

AERODONETICS

Étude Géométrique sur l'équilibre et la descente rectiligne de l'aéroplane. (Lecornu.) "Tech. Aér." Feb. 15

Mémoire inédit à propos de la détermination de la résistance de l'air sur les surfaces en mouvement. (A. Pénaud.) "L'Aér." Jan. 22, Feb. 12-19

AERODYNAMICS

Les expériences de M. Eiffel sur les surfaces sustentatrices. (Espitallier.) "Tech. Aér." Feb. 15

Les expériences de M. Esnault-Pelterie sur la résistance de l'air. "Tech. Aér." April 1

Note sur une expérience récente de M. Rateau. "L'Aérop." April 1

Theorie und Versuchswerte des Luftwiderstandes. (Kraus.) "Flug." April 10

AEROPLANES

Aeroplane de M. R. Saulnier. "Tech. Aér." March 1

Aéroplane Louis Bréguet. "L'Aérop." Feb. 1

Description of the successful types of Aeroplanes. (Loening.) "Aeron." Jan. and Feb.

Essais sur quelques questions d'aviation [Mathematics of Construction]. (Rabot and Besançon.) "Rev. de l'Av." March 1 et seq.

Étude comparative entre les différents types d'Aéroplanes. "Rev. de l'Av." March 1

Farman Aeroplane. "Aeron." Feb.

Herring-Burgess Biplane. "Aeron." April

Pfitzner Monoplane. "Aeron." March

Santos-Dumont's "Demoiselle." "Aeron." Feb.

AEROPLANES: THEORY

Die Steuerungs und Stabilisierungs Einrichtungen der Flugzeuge. (Weber.) "D.Z.L." Jan. 26

Rôle du vent dans l'aviation. (Lorenc.) "L'Aérop." March 15

Stabilisierung der Aeroplane. (Sparmann.) "Flug." Feb. 10 and Feb. 25

Sur un nouveau mode d'équilibrage des Aéroplanes. (Bazin.) "L'Aérop." Feb. 1

The Centre of Pressure on Arched Surfaces. (Sellers.) "Aeron." March

Um welchen Punkt dreht sich ein Flugkörper. (Sparmann.) "Flug." Jan. 25 and Feb. 10

AUTOROTATION

L'ecoulement des filets d'air et les phénomènes d'autorotation. (Étévé.) "Tech. Aér." Feb. 1

BALLAST

Formule générale du délestage. (Do.) "Rev. Aér." March 1

BALLOON FABRIC

Appareil pour l'essai de la perméabilité des étoffes d'aérostats. (Josse.) "L'Aérop." April 1

Epreuve à la traction des étoffes de ballon. (Josse.) "L'Aérop." March 15

Les tissus à ballons. (Taris.) "Rev. Aér." March 10

Tissus droit fil contre Tissus biais. (Echalié.) "L'Aérop." April 7

BIRD FLIGHT

Le secret du vol des oiseaux. (Étévé.) "Tech. Aér." April 1

Poids et surface alaire de l'oiseau. (Thouveny.) "L'Aérop." Jan. 25

DIRIGIBLES

Le dirigeable "Leonardo da Vinci." (Gédo.) "Tech. Aér." Feb. 1

Le dirigeable "Leonardo da Vinci." "Rev. Aér." March 25

Le dirigeable militaire Italien. (Pesce.) "L'Aérop." Jan. 15

Les dirigeables militaire Italiens. (D'Arici.) "Rev. Aér." Feb. 10 and March 1

DIRIGIBLES: THEORY

Die dynamische Höhensteuerung eines Lenkballons. (Horst.) "D.Z.L." Feb. 9 and March 9

ELECTRICITY
Magnetische und Elektrische Probleme für die Luftschiffahrt. (Dieckmann.) "D Z.L." Feb. 23

FLAPPING FLIGHT
Les Battements de l'aile suivant une course en forme de huit. (Constant) "L'Aér." March 26

HEIGHT MEASUREMENT OF AEROPLANES
Sur la mesure de la hauteur des Aéroplanes. (Noel.) "L'Aérop." Feb. 1

HELICOPTERES
Les Papiers inédits du Colonel Renard. Nos. III. and IV. "Tech. Aér." Feb. 1 and April 1
Sur le maximum de poids utile enlevé par des hélices de sustentation. (Sée.) "L'Aérop." March 1

HYDROGEN
L'Hydrogène. (Taris.) "Rev. Aér." Feb. 25
Neue chemische Probleme. [Liquid hydrogen.] (Erdmann.) "Flug." Feb 10
Notes sur la question de l'hydrogène au point de vue de l'aérostation en 1909-1910. (Échalié.) "Tech. Aér." April 1

KITES
Considerations générales sur les Trains de Cerfs-Volants de Guerre. (Casoyen.) "L'Aér.' March 5
Les Cerfs-Volants Montés. (Lecornu.) "L'Aér." March 12

LABORATORY
Le Laboratoire d'essais de M. R. Saulnier. "Tech. Aér." March 15

LAW
Die privatrechtliche Stellung des Ballonführers. (Pfuhlstein.) "D.Z.L." Jan. 12 and Jan. 26
Wright Brothers' suit. "Aeron." Feb.

MAPS
Die Karte des Deutschen Luftschifferverbandes. (Moedebeck.) "D.Z.L." Jan. 12
La Carte de la Fédération des aéronautes allemands. (Moedebeck.) "Tech. Aér." March 15

MILITARY AERONAUTICS
Der Flieger im Heeresdienst. (Olszewski.) "D.Z.L." March 9
Qualités militaires des aéronefs. (P. Renard.) "Rev. Aér." Feb. 25

MOTORS
Ein historischer Luftschiffmotor. (Renard.) "D.Z L." Jan. 26
Les moteurs d'aviation Panhard et Levassor. "Rev. Aér." March 25
Rumpler motor. "Flug." April 10
Sur le volant des moteurs d'aviation. (Lecornu.) "Tech. Aér." Feb. 1

ORNITHOPTERES
Le problème de l'ornithoptère. "L'Aérop." Feb. 15

PROPELLERS
Fonctionnement au point fixe des hélices aériennes. "L'Aérop." Jan. 15
Le Laboratoire d'essais de M. R. Saulnier. [Apparatus for testing.] "Tech. Aér." March 1
Sur la Théorie des hélices aériennes. (Saint-Marcq.) "Tech. Aér." Feb. 15
Sur quelques essais d'hélices au point fixe. (Auclair.) "L'Aér." April 2
Théorie synthétique de l'hélice propulsive. (Rabbeno.) "Tech. Aér." Feb. 1

REGULATIONS
Projet de Règlementation de la circulation aérienne. "L'Aérop." April 1

SOARING FLIGHT
Energie et vol à voile. (Thouveny.) "L'Aér." March 19 and April 9
Formules du vol à voile. (Thouveny.) "Tech. Aér." March 1 and 15

SPEED MEASUREMENT OF AEROPLANES
Correction du vent dans la mesure de la vitesse propre des aéroplanes parcourant un circuit fermé. (Soreau.) "Tech. Aér." Feb. 1
Über das Registrieren horizontaler Luftschwankungen. (Helmhacker.) "Flug." Feb. 25 and March 10

STATOSCOPES
A new principle in the construction of statoscopes. (Custer.) "Aeron." Feb.

TORPEDO
La torpille aérienne. (Lorin.) "L'Aérop." Feb. 15

TRANSPORT OF AERIAL CRAFT
Frachtberechnung für Luftfahrzeuge auf Eisenbahnen. (Richter.) "Flug." March 25